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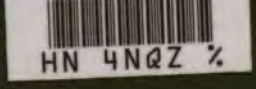

METAMORPHISM
OF
ROCKS

DR. A. IRVING

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METAMORPHISM OF ROCKS.

CHEMICAL AND PHYSICAL STUDIES

IN THE

METAMORPHISM OF ROCKS

BASED ON

A THESIS (WITH APPENDICES) WRITTEN FOR THE

DOCTORATE IN SCIENCE IN THE

UNIVERSITY OF LONDON.

BY

Alexander

A. IRVING, D.Sc., B.A., F.G.S.

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TO THE

Rev. T. G. BONNEY, D.Sc., Ll.D., F.R.S.,

Past President of the Geological Society,
Professor of Geology in University College, London;
Fellow of St. John's College, Cambridge;
Honorary Canon of Manchester;

in admiration of his skill in microscopic petrology and as a field-geologist, and of his extensive culture; this little work is dedicated by

THE AUTHOR.

Prefatory Note.

The inception of this little work is due to the Presidential Address of Professor Bonney to the Geological Society in 1886. When that Address appeared it seemed to me that some of the leading ideas contained in it would admit of a fuller consideration from the chemical and physical side; and in this I was happy to find that the author of the Address concurred. My first attempt to deal with them was in a paper which I hastily put together for the Birmingham Meeting (1886) of the British Association. I found however that even in its incipient stage the subject was too vast to be dealt with satisfactorily in a paper, and I had to content myself with a brief statement of some of the leading points, which appeared in the Association's Report for that year.

In writing the Thesis on Rock-Metamorphism I was fully conscious of many imperfections in the treatment of some portions of the subject. It was especially so with parts of Sections ii and iii, which were written for the most part in 1886. As the other parts and the subsidiary matters contained in Appendix ii grew to considerable proportions, I found that with my daily work and the inroad which two other papers* made upon my time in 1887, I was not able to re-cast Sections ii and iii as I could have wished to do, without risking the delay of another whole year in sending in the Thesis, and this for obvious reasons it would have been unwise to do. These matters were worked out more fully in a supplement, copies of which have been privately distributed along with the Thesis. The Thesis has been submitted to some of the highest authorities in this country and on the Continent; and the friendly acknowledgements it has met with abroad have been to me encouraging in the highest degree. The matter contained in the Supplement is now published at the suggestion of the University Examiners incorporated with the original Thesis, which has undergone careful revision, the alterations being however to a great extent merely verbal. Some few further additions have been made both to the body of the work and to the original appendices; and these together with the matter contained in

* See Q.J.G.S. for May, 1888.

the Supplement have been printed in smaller type. A little delay in the publication has enabled me to draw attention here and there to valuable contributions to petrology made by foreign geologists of eminence in the 'Etudes sur les Schistes Crystallins' published by the International Geological Congress, which met in London, in September, 1888.

It would not be possible, were I to attempt it, to express my indebtedness to Professor Hermann Credner of Leipzig, whose masterly and philosophical work, 'Elemente der Geologie,' as it stands in the sixth edition, (1887), is still without a rival in our language, as a storehouse of geological facts and principles. * It is some gratification to me to find my own conclusions on some of the more important points in connexion with the genesis of the crystalline rocks so thoroughly in accord with those of Thomas Macfarlane, Esq., F.R.S.C., the result in his case of very extensive experience both as a metallurgist and as a field-geologist. It was with the greatest pleasure that I read his 'Origin of the Eruptive and Primary Rocks' (written in 1864) after the earlier sheets of this work were printed off, the author having kindly given me a copy of it at the International Congress in London.

The departure which our University has now made in recognising 'original work' and in doing its part in lifting the discussion of higher scientific questions out of the professional arena is of itself the best reply to the criticisms that have been passed in some quarters, and augurs well for the future of Science in this country. It may be questioned whether the outcry referred to is not to a great extent a reflex of a belief in that system of over-teaching which has done so much to check the growth of original scientific thought in this country, as compared with some of the leading nations of the civilized world, by giving too much advantage in the academical race to mere receptivity. It seems to be forgotten at times that the truest 'teaching' is that which stimulates the mind to active thought, not that which saves the student the trouble of thinking by loading the memory with second-hand knowledge. One necessary consequence of this is seen in the rareness of appeals in a great deal of the scientific literature of this country directly to nature, as compared with the quotation of names of authority as giving weight to certain 'views.' There are reasons for doubting whether, with the exception of the Royal Society (which represents

* Where not otherwise indicated the references to it in this work are to the 3rd Leipzig edition.

all the sciences) there is a single scientific society in London which is entirely free from the interested influence of a close profession. In so far as such an influence is allowed to exert itself there grows up a tendency to fetter the discussion of scientific questions by a spurious 'orthodoxy'; and just so far does such a society come short of the fulfilment of its highest function, which is the advancement of pure science, in the sense of an extension and correction of our knowledge of the material universe.

Truth is indeed 'a pearl of great price,' and not easy to find amid the heap of 'wood, hay, and stubble' of that department of literature which calls itself 'geological.' It is no difficult task for one possessed of a fair amount of training in the literary art to write on natural subjects so as to appear very profound to those who know a little less than he does. Those on the other hand to whom it has been given to experience the regenerating influence of Nature upon the human intellect, will acquit me of all suspicion of cant, when I say that above and beyond any honours or distinctions which it is in the power of any academical body to bestow, nay even beyond and above the appreciation of one's work by one's own contemporaries, research confers its own reward in the healthy habit of mind which it induces. It, and it alone, can teach us to appreciate the sublime beauty of that saying of Lessing's in his 'Streitschriften' with which the President of the British Association closed his Address at Manchester in 1887.

To get a glimpse of new fragments of truth before they have become the fashionable idol of the academic crowd, or the current coin of the examination-room, or the commonplace commodities of the publishing mart, is perhaps the purest pleasure of which on the intellectual side our present organization is capable.

A. I.

WELLINGTON COLLEGE,
BERKS.
1st June, 1889.

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"Nur die überzeugendsten Gründe, also sowohl der Nachweis der Ursache des Umwandlungsvorganges, als die Beobachtung inniger, durch allmählige Uebergänge erwiesener Verknüpfung des umgewandelten Gesteines mit dem ursprünglichen Muttergestein geben die Berechtigung, ein Gestein als metamorphisch zu bezeichnen."

PROF. HERMANN CREDNER;
Elemente der Geologie

ROCK-METAMORPHISM

CONSIDERED FROM THE

CHEMICAL AND PHYSICAL SIDE.

§ i. INTRODUCTION.

In Physical Geology there is no subject more complicated than Metamorphism, nor one which presents to the student of Geology so many vexed and complicated questions. True progress in this direction can only be made by a threefold attack upon the problems which it presents to us. They must be studied—

1. in the field;
2. with the microscope;
3. in the light of known physical laws and chemical principles.

In the first of these three lines of work our own country has undoubtedly taken the lead, but it has been rapidly overtaken by the geologists of Germany, Austria, France, and America.

In the application of the microscope and microscopic methods to petrology it would be admitted, I think, on all sides that we have to thank Germany* mainly for the elaboration of this more exact mode of research; but thanks to the indefatigable energy of several workers—*e.g.*, Bonney, Sorby, Allport, Judd —such rapid strides have been made in this country in the last decade or so that English petrology may be said now to take a place in this respect second to that of no country in the world.

With respect to the chemico-physical side however it would be rash to assert as much. There is some advance here in the latest text books; the physical and chemical sides of many geological phenomena are handled much more freely than they used to be; but there is room yet for much improvement. It is not the "text-book taster" in chemistry or physics who will rise to that masterly handling of such problems which we see in some of the best geological works on the Continent, but the investigator who can bring such living ideas to the consideration of them as can only be acquired through *bonâ fide* laboratory-work.

It is not too much however to hope that, with the new impetus which has been given of late years to laboratory-

* Zirkel's earlier work on the 'Basalt Rocks' (Bonn, 1870), is dedicated to Henry Clifton Sorby, F.R.S., who initiated microscopic petrographical methods.

work at the universities and the increased facilities which are now afforded by our provincial colleges, the next few years will be marked by rapid strides in this direction; and that the day will soon pass away in which we shall find Bischof quoted as a final authority on such questions; or hear a professor of geology excuse himself for not having made himself acquainted with memoirs on interesting and important questions in physical geology on the ground that they are 'too chemical'; or find a distinguished author of a text-book stating, after writing about dolomites, that "we are still quite in the dark as to the exact nature of the reactions by which they have been produced"; or find another eminent, but now deceased, professor invoking the notion of the "spheroidal state" of water to account for paroxysmal explosions at the mouth of a volcano in activity; or the reducing action of heated protoxides of the heavy metals upon steam overlooked—as it generally is—when the suggestion of Davy,* as to one possible mode of production of free hydrogen at the mouth of a volcano, is considered; or the discussion at the Geological Society of the chemical evidence which may bear upon a stratigraphical question a practical impossibility; or a learned President of that same Society attempting to extend our ideas of those differentiated forms of energy which are concerned in the vital activity of living organisms to the building up of a crystalline mineral.†

General and Preliminary.

The principle of Conservation of Energy is a recognition of the fact, that—so far as the material universe is concerned—the sum-total of its energy is a fixed quantity. This energy—when manifested in operation—is differentiated in the various ways or modes to which we apply the term *force*, so that the forces of nature may be defined as *differentiated energy*.

Without entering into the psychical side of the question we may regard *vitality* in a living organism, so far as its physical side is concerned, as a *summation* of *forces*.

When a selective differentiation of the forces essential to vitality takes place in connection with special organs (*i.e.*, differentiated structures), powers or capacities are developed; and a *summation of powers* constitutes that indefinable thing which we call the *individual*.

To give a full account of the individual in any grade of existence we must trace back each intermediate power which is an essential constituent of the individual to those simple elementary principles which we can recognise as *laws uni-*

* See Appendix ii. Note A. † Appendix ii. Note B.

versal. A scientific imagination may be useful in projecting an idea; but to call in that faculty in *support* of an idea, when direct evidence from nature fails us, is—whatever it may be—certainly not scientific. No true advance can be made by such a use of it. No piling-up of opinion upon opinion can establish a truth of nature, unless such opinions rest ultimately upon evidence furnished by Nature herself.

In the conflict of opinions and views which has been waged for years over those phenomena connected with rock-structure which are generally understood to be included under the term "metamorphism" in its more restricted sense, it is to be feared that many "theories" which have been put forward from time to time are simply suspended in the air; instead of hanging on to a series of rigid *inductions from facts* they have often little more than the imaginations of their authors to rest upon.* A really solid basis for theory can only be laid in a careful and laborious observation by impartial minds of the facts presented to us in nature, or arrived at through the experimental work of the laboratory. We must get our ideas—our way of looking at things—our "theories"—by inductions from the hard facts of nature; and this some of our best workers are beginning to realise. But we must not imagine that an inference drawn from *a set of facts of one kind* is of itself sufficient to give a full account of the manifold and complex phenomena presented to us in the more highly (so-called) metamorphic rocks, such as the crystalline schists.

The term "Metamorphism."

The root idea of μορφη is no doubt shape or form. But in scientific nomenclature the idea is limited to *internal structure.* In Botany, for example, there is no difficulty in distinguishing between *morphology* and *external conformation.* The former —the structure and growth of the cell, the mode of elaboration of tissues and organs—is determined by definite laws and the operation of definite forces (not in every case clearly defined, it may be); that is to say, *differentiated forms of energy* applied in Nature's laboratory. The gardener trains a fruit-tree to a wall, and thus alters its external conformation; but no one would be so foolish as to say that the morphology of the plant was changed thereby. It is equally unscientific to attempt to extend the word metamorphism in petrology to such accidental changes in the conformation of a rock-mass (large or small), as may occur, for example, in the indentation of pebbles by

* See Appendix ii. Note E.

the pressure of a harder pebble upon a softer one, as is often seen to be the case in the Nagelfluh conglomerate and elsewhere.

How necessary it is to fix such limitations to the meaning of the term is seen from the fact that a learned professor in this country * only five years ago brought forward a similar case from the Old Red Sandstone Conglomerates of Scotland, as furnishing what he called "an example of an early stage of metamorphism."

In a letter dated Zürich, 1849, Von Cotta tells us that Escher v. d. Linth showed him a great case full of such in the Museum at Zürich. Many of the larger indented calcareous pebbles were split and their cracks filled with calc-spar, just as the cracks in the septaria of the London Clay are filled up. No one has yet ventured to bring forward the latter as instances of 'metamorphism,' nor on the other hand can it be shown, I think, that any continental writer has indulged in such an abuse of the term, although some of them at least—as is shown above—have long been familiar with the phenomena. As well might we call the striations and grooves of glaciated rock, or the work of a mason's chisel upon a block of stone, 'metamorphism,' as to apply that term here.

In this thesis 'metamorphism' will be used to mean *only changes in the internal structure of rock-masses* (*i.e.* in their morphology); everything connected with external conformation, which is purely accidental, is excluded.

In an attempt to deal with the vast subject of metamorphism from the chemical and physical side, as thus outlined, it is not possible within the limits of a thesis to do more than touch upon its more salient points. One thing however must be premised; we are not dealing merely with the so-called 'metamorphic rocks' of the systematist, but rather with *principles*. For this reason it will be best to rise above the text-book level of looking at the facts and ignore the restrictions and limitations which may be convenient and even necessary in classifying rocks. Here as in so many cases we have to recognise the fact that Nature knows no sharp lines of demarcation, though for the conveniences of study and description they are admissible.

Divisions of the Subject.

A year or two ago (see Brit. Assoc. Report, Birmingham, 1886, p. 658) I proposed to exclude from metamorphism in the stricter sense of the word, as defined in this work, all such changes as could not be included under the two terms

* Brit. Assc. Report, Southampton Meeting (1882), p. 536.

"Metatropy" and "Paramorphism." Further consideration of the subject has led me to see the necessity of recognising in slaty cleavage and its concomitant phenomena a kind of metamorphism; and this will be considered under the term *Metataxis*.*

We have therefore to deal with the three following:—

1. **Paramorphism**, including all those changes within a rock-mass (essentially of the nature of chemical change) in which the original minerals have had their chemical composition more or less altered, while new minerals are formed within the mass.

2. **Metatropy**, or changes in the physical characters of rock-masses, while there is no essential chemical change either in the rock-mass or in its constituents.

3. **Metataxis**, or changes of order of the constituents of the rock-mass, of which the phenomenon of slaty cleavage may be taken as a typical instance.

As changes of the first class are *atomic* (chemical), and those of the second-class *molecular* (physical); so changes of a metataxic nature must be considered purely *mechanical*.

We may parallel the three kinds of change with the distinction which is generally and easily recognised in science in the three degrees of divisibility of matter between—

(*a*) the dissociation of a molecule into its several atoms;

(*b*) the breaking up of a solid into a liquid and ultimately into a gas, by elevation of temperature, the cohesion of the mass being partly overcome when it assumes the condition of liquidity, and entirely overcome when it assumes the gaseous state; *i.e.*, when the mass is divided into single and individual molecules;

(*c*) molar division, whether by pressure, percussion or grinding.

The distinction is as clear and as easily drawn in the one case as in the other. Mere alteration of the external conformation of a rock-mass (*e.g.*, flexure) has been already excluded from the term metamorphism, and consequently from each particular division of the subject.

There is yet a fourth class of changes which have often been referred to by many writers as "metamorphic," but which are scarcely admissible within the limits of the term as here laid down. The fact that they have been thus described necessitates some notice of them in the present work; and for these I propose the term "*hyperphoric change*." Such changes as the introduction of a new mineral into, or the removal (wholly

* This term is preferred to the cognate term *Metastasis* (Bonney), since that term has already been appropriated in Morphological Botany.

or in part) of an old mineral from, the original rock-mass (of which the dolomitization of limestones may be taken as typical) fall under this head.

How necessary it is to have a nomenclature for discriminating the various phases and degrees of "Metamorphism" is seen by the frequent confusion of the argument arising from the want of such discrimination in more than one of our recent text-books of physical Geology. Such terms as 'normal' and 'regional' metamorphism do not help us much, since they rather imply that the *cause* we are in search of is known; and they admit of a certain element of vagueness arising from differences in the theoretical views of those who use them. What is wanted is a terminology based on *distinction of kind* rather than on *genetic theories*, and this must be offered as an excuse for the new nomenclature here proposed.

§ ii. PARAMORPHISM OR MINERAL CHANGE.

a. **Primary Paramorphism (genesis of rocks).** It may be well to consider here *in limine* a few of the simpler chemical reactions which have a direct bearing upon this part of our subject, such as the decomposition of an original silicate, from which silica may be deposited. In the formation of dolomite again paramorphic change comes into play to some extent, and there is no valid objection that I can see on chemical grounds to the theory that both the carbonates of lime and magnesia may have been precipitated in some cases in concentrated marine waters, or waters of salt lakes, by chemical reactions.

To account for the enormous quantity of NaCl in sea-water by mere washing-out of this mineral from the earth's crust would be to reason in a vicious circle: it is more likely that it has resulted partly from the action of more strongly electro-positive elements of the alkalies (in this case mainly sodium) taking up the strongly electro-negative chlorine from the soluble chlorides of the alkaline earths and other bases. Thus sodium or potassium dissolved out of felspar by carbonated waters would be carried down to the sea in the form of *soluble carbonates*. These would then re-act upon any chlorides of the alkaline earths present thus—

$$Na_2CO_3 + CaCl_2 = 2\ NaCl + CaCO_3;$$
$$K_2CO_3 + CaCl_2 = 2\ KCl + CaCO_3.$$

In such cases there might be a direct precipitation of $CaCO_3$ to form limestone, *and such reactions in the earliest condensed waters may have been the origin of the bands of marble* which are met with in the crystalline schists of the Alps and elsewhere.

The decomposition of silicates by corbonated water is clearly not the only source of the alkaline carbonates. If we consider the pyrogenic stage of the formation of minerals in an early stage of the earth's lithosphere, and take into account the high degree of stability which the carbonates of the alkalies

manifest (*infra*) even under our present atmospheric pressure, there seem strong grounds for assuming that under a much greater pressure and at higher temperatures the alkaline carbonates would be freely formed in the dry way by direct synthetic combination of CO_2 with the oxides of the alkali metals, which, having a far higher degree of stability than the carbonates, would have been already formed at a still earlier stage.* The avidity with which halogens (in different degrees) attack metals, the great thermal stability of their chlorides for the most part and to some extent also of their bromides, the differentiating factor of the non-solubility of many of the fluorides, together justify us in assuming the antecedent formation of a much greater variety and abundance of haloid salts (especially of the chlorides) in the earliest primæval waters of the globe.†

Such considerations lead us to regard as concomitant and correlated phenomena‡ the occurrence of bands of crystalline limestone among the true crystalline schists of the Alps (of which I speak from personal knowledge) and elsewhere and the initiation of that *salinity* of oceanic waters with which we are familiar, long antecedent to the earliest reprecipitation of Na Cl, KCl, &c., from them at later stages by local concentration. And further they offer, I venture to think, a more sufficient explanation than any I have yet met with of the occurrence first noticed by Sorby of the presence of "chlorides of potassium and sodium, and even of sulphates of potash, soda and lime" in solution in the water of the 'fluid cavities' of the quartz of granite; traces of these salts being present in the water included in the quartz during the original cooling of the granite. In this sense we may perhaps understand that distinguished observer's conclusion that it was "a genuine constituent of the rock when melted."

The precipitation of Ca CO_3 to form bands of marble in the schists, is not to be understood as implying condensation of water on the crust on any large scale. Such water as then existed must have formed mere *puddles* as compared with the *oceans* of later time. Ca CO_3 and Mg CO_3 were no doubt

---

* It will be recollected that the highest artificial temperatures obtainable are incapable of dissociating the oxides of the alkalies and alkaline earths.

† See further Credner, *Elem. der Geol.* (6th ed). p. 321.

'The non-solubility of many of the fluorides.'

In 100 parts of water—

Ca F_2 is dissolved to the extent of ·037 parts.

Sr F_2 rather more.

Ba F_2 rather more than Sr F_2.

The solubility increases here (as is so often the case) with the chemical activity of the base. Take again the fluorides of the magnesium group of metals (MgF_2, ZnF_2, CdF_2, BeF_2): of these only BeF_2 is at all freely soluble.

‡ That is to say, co-ordinate products of the same chemical reactions.

produced in early archæan times both by wet and dry reactions, though both of course at high temperatures. The case of the calcareous schists of the Glockner Group (in which the calcite is interfoliated with the quartz and the mica) may perhaps serve as an instance of the formation of $CaCO_3$ in the wet way, and possibly by the action of free CO_2 in solution, since the large quantity of free quartz present seems to point to this.

An analysis in my laboratory of the specimen of marble from the schists of the Alps (the Valser Rhein, see App. ii. Note H.) gave 27% of siliceous and earthy material insoluble in HCl. The specimen itself shows traces of a sort of bedding-lamination both macro- and microscopically. Under the micr. a thin slice exhibits opaque (earthy) material with a more or less linear arrangement in a clear field, the latter transparent in ordinary light, but breaking up into the finer crystalline-granular texture of 'körniger Kalk' between crossed nicols. Its composition and texture thus combine to point to its probably resulting from the reaction of an alkaline carbonate upon a silicate containing lime and alumina as bases, the CaO taking up its equivalent of the CO_2 while the Al_2O_3 was separated out, from its inability to take up CO_2 under the existing conditions. The marble occurs in lenticular masses.

In the earliest stages of the earth's history the chlorides may have been formed by direct combination of Cl_2 with the metals; but, looking at the rapidity with which Cl_2 replaces O in H_2O at red heat, we may regard it as far more probable that most of the Cl_2 (and almost certainly the F_2) combined directly with free H_2; since this could take place at a higher temperature than $H_2O + Cl_2 = 2HCl + O$. As condensation advanced H F and H Cl dissolved in H_2O would attack the oxides, silicates and carbonates of the metals and furnish another abundant source of the chlorides in the earliest marine waters.

Prestwich's idea of the slow transformation $[H_2O + Cl_2 = 2\ H\ Cl + O]$ of *chlorine-water* under the influence of light is true as far as the chemical fact goes, but scarcely admissible under the high-temperature conditions we are considering.

Probably both Br_2 and I_2 (which can be made to combine directly with H_2 by heat and contact-action) united in the first instance also directly with H_2; their haloid salts in sea-water resulting from reactions of the acids HBr, HI upon oxides, sulphides, carbonates, and silicates of the alkalies, alkaline earths, and magnesium.

But we need not stop here. Carbonate of lime formed as above suggested, containing, as it does, a more strongly electropositive base (calcium) than magnesium, its metallic base would replace that metal, forming haloid salts of Ca, in the place of corresponding haloids of magnesium, in the earliest waters, thus—

$$Ca\,CO_3 + Mg\,Cl_2 = Ca\,Cl_2 + Mg\,CO_3;$$
$$Ca\,CO_3 + Mg\,Br_2 = Ca\,Br_2 + Mg\,CO_3.$$

($Mg\,Br_2$ it will be recollected is the chief salt in "bittern" from which bromine is obtained). The haloid of calcium would be removed in solution and the $MgCO_3$ precipitated. These things happening together and magnesium being remarkable for its readiness to form double salts (which is a chemical fact), there ought to be no difficulty in seeing how at a later stage the double carbonate (dolomite) may in some cases have been produced as the direct result of simultaneous chemical reactions, and even as a result of the reactions of the salts of sea-water upon a pure deposit of carbonate of lime. Magnesium

sulphate in sea-water may react equally well on carbonate of lime, thus

$$MgSO_4 + CaCO_3 = MgCO_3 + CaSO_4.*$$

This may explain how it is that gypsum is so frequent an accompaniment of dolomite as it is in the Zechstein of Germany. A study of the Durham coast-section in the summer of 1886 forced upon my mind the idea, that much of what is observable there in the rock-structure on the face of the older parts of the cliff, so different from what is seen in new exposures of *the same beds* in quarries very near the coast, is the result of changes partly hyperphoric partly paramorphic, produced by the long continued action of the spray of the sea. It throws some light too, I venture to think, upon the occurrence of dolomites as well as marbles among the Alpine schists. Of course it will be readily understood that these and analogous reactions may be produced by salts held in solution, in mineral waters. In this way local deposits of dolomite may have been produced. Later experiments of Dr. Sterry Hunt have removed the difficulty which was raised by the experimental failure of Bischof in his attempts to precipitate pure dolomite. The former investigator finds that they unite together to form dolomite at the moment of their formation, though magnesite could not be taken up in this way by bi-carbonate of lime in solution. The formation of pure dolomite as a chemical precipitate is thus seen to be an illustration of the principle which has gained recognition in modern chemistry, that *radicles* as well as atoms have a special degree of activity in the 'nascent state;' since at that moment the system of which a radicle consists has had its most stable form of configuration disturbed, and so allows more independent action of its constituent atoms.

It is more probable that $MgCO_3$ was in such cases formed simultaneously with $CaCO_3$ by direct action between alkaline carbonates and haloids of Mg as well as Ca; for this simply assumes the presence of (*e.g.*) $CaCl_2$ and $MgCl_2$ together in the same concentrated waters. This would give us the antecedents necessary for Sterry Hunt's reaction mentioned above (p. 9). The more intimate admixture of $MgCl_2$ and $CaCl_2$ in a concentrated solution would render this process far more likely than that $MgCa(CO_3)^2$ should have been produced by the direct action of CO_2 upon the insoluble silicates of Mg and Ca. (*infra*, pp. 12-13). There is also some difficulty in postulating the action of $MgSO_4$ upon $CaCO_3$ in the early archæan times, on account of the (thermal) instability of most of the sulphates, though this manifestly raises no difficulty in a modern case like that of the Durham cliffs, nor does it cause any difficulty in attributing archæan dolomitization to reactions of haloids of Mg upon $CaCO_3$ as shown in the equations given on page 8 of this work It is not improbable that the removal of a large amount of Mg from the waters of the ocean during the vast interval between the Old Red Sandstone and the Jurassic periods and the storage of it in the dolomites, by the reactions mentioned on pages 8 and 9, may have done much to render the sea

* cf. Sorby, Q.J.G.S., vol. xxxv. p. 73. Such experiments would be more satisfactory if checked by analyses of the solutions afterwards.

fit to support the teeming life of the Mesozoic ocean. I have verified these by laboratory-work, and found

(1) That when the two mixtures were allowed to stand for two or three weeks in distilled water, with occasional stirring, very small quantities of Ca were found in the alkaline filtrate: but

(2) That on boiling for a couple of hours ($CaCO_3$ in excess), Ca was copiously precipitated from the alkaline filtrate. [A 'blind experiment' was then made with complete negative results, in order to guarantee the absence of Ca in the filtrate from any decomposition of $CaCO_3$ by mere boiling].

The condensation of the water of the Dead Sea (Sp. Gr. at a depth of 300 metres = 1·25, yielding 27·8 % of solid residue) in which carbonates are absent, and the salts in solution are almost wholly composed of haloids of K, Na, Ca, Mg, is a remarkable fact. Had the physiography of the Jordan basin been such as to charge its water more freely with alkaline carbonates the large proportion (Roth, *Allgem. u. Ch. Geol.*, p. 478) of haloids of Ca and Mg now suspended in its water would doubtless have been precipitated as carbonates to form dolomite.

That class of paramorphic mineral changes by chemical reactions, of which the formation of dolomite may be considered one of the simplest examples, would include, as Credner remarks (*El. der Geol*, 3rd ed. p. 295), the ultimate conversion of rocks containing augite, mica, hornblende, garnet, diallage, olivine and chondrodite, and especially of eklogite, olivine-rock, diabase, gabbro, diorite, hornblende-schist into *serpentine*. This happens, he goes on to say, not merely as a residuum from the processes of decomposition and removal in solution by the action of carbonated waters, but chiefly through the separation-out from solutions of carbonate sulphate and chloride of magnesium, as soon as these come into contact with *silicates* of the alkalies, alkaline earths, and alumina. In all these *the chief agency would seem to be heated or superheated water acting through a long period of time:*

1. as a direct solvent for the extraction of certain salts from the original minerals;

2. by furnishing, as the result of the first, saline solutions capable of acting upon other minerals either by direct synthesis or by double decomposition. See § ii. (β).

Thermal Chemistry (as exemplified below in the carbonates of the alkalies) would seem to throw some light upon the order of succession of primary paramorphic changes from several considerations, of which some may be here suggested:—

1. The law that *most heat is set free in the formation of the most stable compounds*. In so far as this heat escapes there is a *dissipation of energy*, the heat lost representing an equivalent abstraction from the total of the potential energy of chemical affinity of the terrestrial mass. There must have been therefore a gradual transition from the less stable to the more stable forms, and when these were ultimately reached no further paramorphic change could occur without alteration of physical conditions.

2. The *great stability of the silicates* points to a high degree of probability that in the secular cooling of the earth's original non-differentiated mass they were among the *earliest compounds solidified*, extensive consolidation of the *metals* * having preceded even this stage; and generally we may say that the relative stabilities of individual minerals of the earth's crust must have been the prime factor in determining a general progressive order in the deposition of them. The carbonates, on account of their comparatively low temperatures of dissociation, must have been among the later.

3. The high temperature (that of white-hot platinum) at which *steam* is only partly dissociated † under the pressure of our present atmosphere shows that *the oxidation of hydrogen must have taken place at a very early stage to form steam immensely superheated under the pressure of an atmosphere many times greater than that of our present gaseous envelope*, and at a period long prior to the first liquefaction of that body on the globe.

Here then were the conditions, heat, gaseous water, pressure, favourable to paramorphic changes in the first-formed silicates, and giving, it would appear, strong support to the view lately propounded by Prof. Bonney § as a result of field observation and microscopic work, "that the crystalline schists (as he has limited the meaning of the term) and gneisses were formed in archæan ages under conditions which have never subsequently occurred in any large area of the globe." Again as secular cooling went on, and the CO_2 began to be fixed, *carbonates of the alkalies* would be formed, later on those of the alkaline earths, carbonates of the heavy metals being formed at a much later stage. This is a necessary deduction from the known relative stabilities of the carbonates; those of sodium and potassium melting at a red heat, and that of potassium subliming in stronger heat, and at the same time undergoing dissociation only to a slight extent; while the carbonates of the alkaline earths (of which that of lime, the least stable, undergoes dissociation at the temperature of the kiln under the pressure of one atmosphere) are intermediate in stability between those of the alkalies and those of the heavy metals. Now we know from laboratory experience that carbonates of the alkalies decompose silicates readily by dry fusion (converting these in some cases into the corresponding carbonates); and it is probable that as a result of such reactions the first carbonate of lime made its appearance upon the globe.

* See Appendix ii. Note C.
† See Appendix ii. Note A.
§ Pres. Address, 1886.

The formation of carbonates of the alkaline earths when their silicates are fused with alkaline carbonates we should expect to occur from the fact that at a dull red heat carbonate of lime is formed by direct synthesis [$CaO + CO_2 = CaCO_3$]. This can easily be proved on heating a pellet of lime in an atmosphere of CO_2 over mercury, by the loss of volume of the CO_2 and its liberation subsequently by stronger acids.

Wislicenus (*Anorg. Chem.* § 378) says, "Kohlensaure Alkalien bilden mit ihnen (silicates) beim Schmelzen—unter Entwickelung von Kohlensaüregas und Abscheidung von Oxyden *oder Carbonaten* der Metalle—in Wasser lösliche Alkalisilikate;" and further that this is "nur vollkommen durchführbar, wenn die betreffenden Silikate auf das feinste pulverisirt sind."

I have lately examined this point a little more minutely in my laboratory, with the following results:—

(1) Fusing together (until evolution of CO_2 ceased)

(*a*) $MgSi\,O_3 + Na_2\,CO_3 + K_2\,CO_3$;

(*b*) $Ca\,Si\,O_3 + Na_2\,Co_3 + K_2\,CO_3$;

dissolving and completely washing away the alkaline silicates and the excess of the carbonates until nothing but distilled water went through the filter; the residue thus completely washed gives off CO_2 with considerable effervescence when thrown into water and treated with a few drops of HCl.

(2) As an instance of what occurs with silicates of the less active bases of the heavier metals, native silicate of copper (Kieselmalachit) was subjected to the same treatment, the result being that the whole of the copper separated out as oxide, as could be seen by the total absence of the green colour of the carbonate in the washed residue, and its failure to give off CO_2 on treatment with HCl.

On general grounds it is conceivable that under a much greater pressure (say 100 atmospheres, which is not an extravagant range to take for the atmospheric pressure of early archæan time), preventing the escape of the CO_2 as it is replaced by the SiO_2, even carbonates of the heavy metals might be formed; and with a much more moderate increase of the present atmospheric pressures we should expect the proportion of $CaCO_3$ and $MgCO_3$ by dry fusion to be largely increased.

Wet and Dry Reactions.—The important difference between 'dry and wet reactions' is brought out so clearly by our experience of the behaviour of the silicates, that this furnishes a good opportunity for emphasizing it.

(1) We know perfectly well that free CO_2 at ordinary temperatures and in the presence of water replaces SiO_2 freely (as in the kaolinization of the felspars) converting alkaline silicates into carbonates: on the other hand, at fusion-temperatures (in the absence of moisture) this is reversed, and free SiO_2 replaces CO^2 freely. This is seen to be an excellent instance of the importance of 'physical conditions,' in determining the most stable form of a mineral complex, as pointed out on p. 10.

(2) In the wet way, even with prolonged boiling (two or three hours), free SiO_2 does not always replace CO_2 in alkaline carbonates, though in some of its allotropic modifications, as shown by my own studies (described further on in this work) SiO_2 does so undoubtedly to a slight extent. But in double decompositions at fusion-temperatures the SiO_2 passes over from a silicate of a comparatively feeble base to an alkaline base, even to the replacement of CO_2 of the carbonate; the CO_2 escaping or uniting with the weaker base if that is fairly strong, and perhaps with oxides of some heavy metals if confined by pressure until the temperature of the mass is lowered below the dissociation-temperature of their carbonates.

Now if we have regard to *dry fusion*, whether deep in the canal of a volcano or as going on generally at and near the surface of the globe when its outer sub-atmospheric lithosphere was for the most part in a glowing-liquid condition (such traces of H_2O as might be present being in the super-heated condition of a *dry gas* and therefore not interfering with the conditions requisite for dry fusion) we see at once the important differentiating function

of heated (molten) SiO_2 in abstracting bases (especially CaO,MgO,FeO) from such carbonates of those bases as might exist in (or be furnished to) the mass before 'fluxing'; and so a rational explanation is furnished of the extremely silicious nature which both volcanic rocks and the earlier archæan rocks (formed probably in the absence of wet water) possess in common.

On the other hand, after the cooling-down of such masses, and the access of liquid water containing free CO_2, this process (as noticed in § ii. β) was reversed: CO_2 replaced SiO_2 in silicates of the alkalies, alkaline-earths, and MgO; carbonates of these bases were formed as substitution-products (removed or deposited according as they were soluble or not in water) with the separation-out of SiO_2, at first hydrated, but ultimately passing into true quartz with loss of H_2O.*

Of course the same temperature is not required in all cases for 'dry fusion'; this will be largely affected by the proportions of good 'fluxing' bases present. Much information of great importance to the petrologist is given on this subject in the volume *On Fuel* of Dr. Percy's 'Metallurgy' pp. 60—75.

A very good example of the reaction

$$CO_2 + Na_2SiO_3 = Na_2CO_3 + SiO_2$$

by the action of atmospheric CO_2 recently came under my notice, and I dare say the thing has been often noticed before. Some solution of Na_2SiO_3 was left at the bottom of a bottle, and the water gradually evaporated away in the air of the laboratory. As this went on the walls of the bottle were corroded by the separated SiO_2 acting upon the glass of the bottle, very likely to make good some deficiency of the SiO_2 required for the complete chemical saturation of the weaker bases (Al_2O_3 & FeO) in the bottle glass, (which generally contains under 60% of SiO_2), and causing surface-devitrification of the glass. The mass of the silicate when dry was found encrusted with a fine growth of crystals, which by their free solubility in water and evolution of CO_2 on treatment with a few drops of HCl proved to be Na_2CO_3. (See § iii.)

When such conditions were reached that steam could liquefy and CO_2 could be held in solution in water, this water, becoming highly charged with the gas under the then atmospheric pressure, would attack the silicates of the alkalies, taking up those strongly positive metals as soluble carbonates, with *deposition of silica* (SiO_2) *to form the quartz, which appears in such quantities in the archæan gneisses and schists.* These carbonates of the alkalies in solution, when brought into contact with haloid salts of the alkaline earths and the heavy metals, would further contribute to the formation and precipitation of their carbonates by such reactions as are given on page 6.

The earliest terrestrial waters are spoken of in the Thesis as being highly charged with CO_2 'under the then atmospheric pressure.' This is not quite accurately stated. Bunsen's investigations, with which most students of physics are familiar, have established the law that where a liquid (in this case water) is in contact with a mixture of gases, the weight of each gas (for a given temperature) dissolved is proportionate to *the pressure of that gas.* Of the existence of a much larger proportion of CO_2 in the archæan atmosphere there can be no doubt. This point has been so strongly put by Credner, (*Geologie* 6th ed. p. 321) that I quote his own words: "In den ältesten Perioden waren die atmosphärische Niederschläge kohlensäure-reicher als jetzt, da sie ihren Weg durch einen Luftkreis machen mussten, in welchem sich die ganze

* cf. J. A. Phillips 'On the History of Mineral Veins' (Q.J.G.S., vol. xxxv.)

Kohlensäure, die heute den Karbonatgesteinen als solche, in den Pflanzen und Kohlengesteinen, sowie im Graphit als Kohlenstoff, im Bitumen als Kohlenwasserstoff der Erde einverleibt ist, noch in gasförmigem Zustande verteilt befand. Diese an Kohlensäure reiche Regenwasser mögen auf die Kalk-und Magnesiasilikate der Erstarrungskruste in hohem Grade zerlegend eingewirkt und dem Meere stark konzentrirte Solutionen von Kalk- und Magnesiakarbonaten zugeführt haben."

In Note L (App. ii) I have shown how our laboratory-experience leads us to see the probability of the production of the archæan graphite by dissociation of hydrocarbons; but with this exception we may accept the idea contained in this passage in its entirety. The same writer points out how the absence of organisms capable of secreting carbonate of lime from these primeval waters would be favourable to their local concentration; he also notes that the water of the present ocean requires evaporation to the extent of 75% before precipitation of $CaCO_3$ sets in.

Accepting Bischof's statement "that the carbonate of lime of all the formations would form a layer over the Earth 1000 feet thick," Pfaff (*Geol. als Ex. Wiss., p. 162*) has calculated that the CO_2 contained in it alone would in the free state give a pressure of 356 atmospheres at the surface of the Earth. This is probably excessive if in the calculation the force of gravity has been assumed the same as we know it.

The action of CO_2 appears to me to have reached its maximum in later archæan and earlier palæozoic times, the grauwacke of the former forming perhaps a connecting *physical* link (with its authigenous mica) between the archæan crystalline rocks and the true sedimentary palæozoic series. This, I find, has been recently pointed out by Kalkowsky (*Elemente der Lithologie, p.* 280.) "Dass diese Gesteine (archäische Grauwacke, including 'Glimmertrapp') eine Brücke bilden, die uns zu den holokrystallinen archäischen Gesteinen hinüberführt, so dass über diese neues Licht verbreitet wird, ist augenscheinlich."

I have adopted the terms 'authigenous' and 'allothigenous' from Kalkowsky (*Lithologie*, p. 13) because they seem exactly the words wanted. 'Endogenous' and 'exogenous' are terms that appear to have found favour in some quarters in this country; but the two serious objections to their use in this sense are, (1) that the connotation they bring with them from morphological botany is misleading: (2) they have been appropriated in a different sense in lithology (see note to § vi).

May we not account for the occurrence of the authigenous micas and the garnets in the cement of these grauwacke as resulting from the combination of the nascent SiO_2 (replaced by free CO_2 as suggested in this work p. 13) with the requisite bases sparingly present, while the large excess of the SiO_2 went to form the bulk of the grauwacke?

Such considerations point to a very unequal distribution of the *silicates* in the earlier crust, as well as marked localization of the earliest condensed waters; but the action of free CO_2 may be regarded as perhaps the main factor in determining the distribution of the earliest limestones and quartzites. Where the CO_2 attacked $MgSiO_3$ or $CaSiO_3$ (or both) impure (siliceous) carbonates would be formed; but where Na_2SiO_3 or K_2SiO_3 were chiefly present the carbonates formed would be removed in solution, and the SiO_2 would furnish the mass of the material of a grauwacke.

The distribution of the limestones and quartzites (if such was the mode of their genesis) shows the continued action of free CO_2 as a very potent paramorphic agency in earlier palæozoic times; while the theory dispenses entirely with all necessity for postulating the existence of living organisms as agents concerned in their production.

The fact that excess of CO_2 in solution in water causes $CaCO_3$ to enter into the form of the soluble bicarbonate $CaH_2(CO_3)^2$ may appear *prima facie* to be a fatal objection to the theory that limestones *per se* could be formed in any quantity by the direct action of carbonic acid on the lime- and magnesia-

silicates; but the objection loses its force when we consider the further fact that the bicarbonate is resolved into the normal carbonate, with separation-out of CO_2 and H_2O below the ordinary boiling-point of water (100°C.), and even by concentration of the solution by slow evaporation at ordinary temperatures as in the formation of stalactites. The physical conditions which cause decomposition of the bicarbonate would prevent its formation.

From such considerations it is clear that we have no right to interpret the *crystalline texture* of archæan and palæozoic limestones ('marbles') as *of itself* furnishing proof even of extensive metatropic change; since their crystalline character may be *diagenetic* rather than 'metamorphic.' Even the crystalline character of the limestones is thus seen to afford a very feeble support to theories of 'regional metamorphism'; and it has also been shown that the quartzites are not of necessity metamorphosed 'clean sandstones.' The determination of these alternatives in any given case must clearly turn mainly upon the microscopic examination of the rock; it is enough for our present purpose to show that an alternative explanation is possible.*

The order

$Si\,O_2$, the non-metallic oxide of *silicates*.
CO_2 „ „ *carbonates*.
Halogens, the non-metallic elements of *haloids*.
$S\,O_3$, the non-metallic oxide of *sulphates*.
P_2O_5 „ „ *phosphates*.

gives the general relative electro-chemical energies of the chief acid constituents which enter extensively into the formation of those mineral salts which are widely distributed in the crust of the earth as rock-constituents.

It is not difficult to understand how individual atoms of the alkali metals may have passed through a cycle of changes, aided by the ready solubility of their salts in water and the facility which this fact furnishes for their transport locally. Having first entered into combination with $Si\,O_2$ at a glowing heat, to contribute to the formation of the earliest portions of the crust by forming (thermally) stable silicates, on the solution of these silicates later on in superheated terrestrial waters they may have taken up in succession the other acids in the order of their electro-negative energies, at each stage of change rejecting the feebler acid (to be taken up or not, as the case might be) to enter into a more (chemically) stable state of combination, heat being evolved at every step.

The importance of the part played in promoting paramorphic mineral change *by solutions of the alkaline silicates*, whether formed by the direct action of superheated water or by reaction of carbonates of the alkalies upon the silicates of other metals, may be seen from the following examples of reactions cited by Credner (*op. cit.* p. 198 *et seq.*):

(1) decomposition of the sulphates and chlorides of lime and magnesia with the formation of their silicates;
(2) decomposition of the NaCl by silicate of potash with production of Sylvine (KCl) and silicate of soda;
(3) decomposition of bicarbonate of magnesia with formation of silicate of magnesia;
(4) decomposition of ferrous carbonate with formation of ferrous silicate;

* While the above was passing through the press I read Prof. R. D. Irving's paper, "Is there a Huronian Group?" (Am. Jour. Sci., Nos. 201-203). The facts recounted by him in his description of the Huronian series proper furnish apt illustrations of the view expressed in the above paragraph.

(5) decomposition of bicarbonate of lime by silicate of soda, the semi-replacement of the sodium by the calcium leading to the deposition of quartz as a pseudomorph after calcite.*

The whole subject is full of interest to the student who can bring chemical ideas to the study of petrology. It must be borne in mind that *all normal salts of the alkalies are soluble in water*, so that the new salts which those bases form are readily transferred by water.

It will be seen that the suggestions here thrown out as to the part played by superheated water and solutions of mineral salts in promoting paramorphic change, taken in connection with principles learnt from thermal chemistry, amount to something quite different to the theory of 'hydrochemical metamorphism,' over which more than one eminent geologist of a generation now passing away has too blindly followed Bischof. Hydrochemical action explains much, but not everything; and in order to get a correct idea of its sphere of action, we must carry our minds back to a remote past in the history of our planet, when aqueous precipitation from the atmosphere was something very different from that of the oxygenated and carbonated waters of the atmosphere of later times upon which Bischof's theory mainly depends. If the archæan gneisses and schists had been formed by the latter agency at such enormous depths as the extreme hydrochemical theory supposes, the time occupied in their 'metamorphosis' is represented by the whole of geological time from the Silurian age down to the present; since "all the formations (as Credner remarks, *op. cit.* p. 307) from the Silurian to the present are found, *wherever their normal types can be studied*, without that extreme phase of 'metamorphism' which characterises a true schist." And there are the further insuperable objections found in the following facts:—(1) The fact that all the palæozoic formations, and especially those of the Cambrian System, *contain rolled and worn fragments of the archæan schists and gneisses in their conglomerates*, which fragments had their several mineral characters at the time of their deposit in their present habitat; though they, together with the strata which now contain them, and the primordial schists and gneisses from which they were derived, have undergone further changes of a metatropic and metataxic rather than of a paramorphic character—in the further metamorphism of so-called metamorphosed rocks. We have to thank Bonney for elucidating this last point in one of his recent contributions to science,† though the idea in a crude form must have forced itself, I think, upon the minds of

* $2\,Na_2SiO_3 + H_2Ca(CO_3)^2 = 2NaC\,O_3 + CaSiO_3 + H_2O + SiO_2$

† Presidential Address to the Geological Society, 1886.

other thoughtful observers of Alpine rock-phenomena, as it has upon my own. (2) There is the fact of *unconformity* which is often very marked and of such frequent occurrence between the archæan schists and the palæozoic formations.

Such evidence goes a long way to show that the essential features of the fundamental gneisses and schists—features which give a general character to the pre-Silurian groups of strata often over 30,000 metres in thickness, wherever they occur, in India, Scandinavia, in Canada and in the Alps—are such as are connected with the *crystalline form in which they were originally consolidated under conditions of temperature and pressure very different from those which now prevail at the surface of the globe;* and that the process of metamorphism so-called, which distinctly characterises the pre-Silurian formations, was at the entry of the earth into the Silurian Period already completed, and could not therefore have occupied the enormous period of time demanded for it by the advocates of the extreme hydrochemical theory. Were it otherwise, all our palæozoic formations must long ago have been converted into crystalline schists and gneisses (Credner, *op. cit.* p. 307). Comparing this with the conclusion at which Prof. Bonney has independently arrived—as published in his address of February, 1886—we may congratulate ourselves as students of physical geology that the more extreme theories of the Huttonian school are breaking down, and that our foremost British workers are beginning to join hands over this question with those of the continent as represented by Gümbel, Credner, Pfaff,* and others.

The term 'Silurian' which occurs several times on this page is to be understood (in the sense in which it is now used by some of the continental writers) as *including the Cambrian.*† This is not surprising when we recollect the prominence of the author of the former term, and the difficulty which most continental geologists must have felt in deciding upon the merits of the great Murchison-Sedgwick Controversy. Even Ramsay seems to have found a difficulty in sharply dividing the Cambrian from the Lower Silurian. (See *Phys. Geol. of Gt. Britain*, 5th ed., 1878, Table of formations, p. 30).

'The further metamorphism of so-called metamorphic rocks.' As illustrations of what is here intended the following references are given :

In Prof. Bonney's Presidential Address (1886);—

Fig. 1. Lepontine gneiss with subsequently-induced cleavage across the original bedding.

Fig. 2. Calcareous mica-schist with similar cleavage.

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* Pfaff's critical analysis of the arguments on which the advocates of 'regional metamorphism' rely is well worth the attention of every student of physical geology. (*op. cit.* pp. 145 *et seq.*)

† Prof. R. D. Irving *(op. cit.)* has shown plainly enough that the true Huronian has a basal conglomeratic member as the Cambrian has. Making allowance however for pre-Cambrian tidal action, I do not think we are bound to follow him in his deductions as to the space in geologic time represented by the Huronian with its 18,000 feet of strata.

In both cases filmy white mica is found on the cleavage-planes, and the phenomenon is designated 'cleavage-foliation.'

Metataxis is recorded in the wavy contortions of the original foliation-planes and the approximation of the flakes of mica to a parallelism with the cleavage-planes.

Figs. 3 and 4 also furnish excellent examples of subsequent metataxic alteration.

With these we may compare the statement of Allport (Q.J.G.S., vol. xxxii, p. 426) that "there is evidence to show that the slates of the Cornish Peninsula existed as metamorphic rocks (*i.e.* cleaved and in places contorted) long before the intrusion of granite. There the contact metamorphism extends to a short distance only (quite distinguishable by modern methods of research from the previous metamorphism) the transition from the one to the other being very gradual."

The importance of the unconformity referred to on p. 17 has been emphasized by Prestwich ('Geology,' vol. i, p. 419).

(1) The passage 'by insensible gradations' of the 'early gneisses' on the one hand into granite and on the other into mica-schists, with which crystalline limestones quartzites and iron-ores are instratified, is contrasted with

(2) the fact that "between the Archæan rocks and the succeeding Cambrian series there is in Europe as well as in America a *marked break of continuity.*"

How disappointing it is in connection with this subject to turn to even the more recent of our text-books written by our foremost geologists, may be seen by noting that in Geikie's Text-book of Geology the archæan crystalline schists are dismissed within a page and a half (pp. 588, 9). The foregoing facts and arguments appear to lie altogether outside the mental horizon of the distinguished author of that work. It is entirely to miss the point to talk of these rocks having been formed "during a period of the earth's history when the *ocean* had a considerably different relative proportion of mineral substances dissolved in its waters"; or, again, to speak of "the same order (of chemical precipitation) having been followed everywhere over the floor of the *ocean*"; and that for the simple reason that the theory requires, and deduces from known facts and principles, a prevalent set of physical conditions under which the 'ocean' *could not possibly have existed as such* * The vast range of temperature between the first initial oxidation of hydrogen † and the condensation of steam into water on any general scale must have afforded ample space in time in the cooling-down of the original nebulous mass of the globe for the formation and deposition of silicates and the separation-out of the quartz, which together make up the granitoid rocks, the gneisses, and the crystalline schists; while on the other hand the high degree of stability of most of the silicates removes all difficulty in the way of their formation while, as yet, the temperature was too high for the condensation of water in anything like *oceanic*

* cf. Pfaff, *op. cit.* pp. 25 *et seq.* † See Appendix ii. Note A.

proportions. These considerations lead us to contemplate rather a condition of things in which deposition on a vast scale was going on from the then dense atmosphere, so as to form a growing *non-consolidated lithosphere on the surface of the globe—an universal hot magma*, which by gradually crystallizing would leave traces of a sort of *bedding* arrangement of its materials from differences of the specific gravity of the minerals formed,* while the mechanical process of *stratification in aqueous basins* (as the term is generally applied) would be out of the question. Such a view, when allowance is made for contemporaneous paramorphic change by chemical reactions between the minerals, appears to be a partial explanation of the banded structure of the archæan schists, when viewed on a large scale, and the occurrence in them of quartz-layers often of considerable thickness.

A similar incapacity for comprehending the theory here advocated appears in Green's 'Physical Geology' (pp. 426-27.) Nothing is easier than to dismiss as 'dreamy conjecture' what we do not comprehend; and Mr. Green's misconception of the theory is shown by his attempt to state it in the following words: "The constituents of those rocks were supposed to have been held in solution in an *ocean of boiling water*, and to have been precipitated as it cooled."†

I submit that the argument advanced in this section of the present work proceeds by way of induction from known facts; and that the further fact that there is a tendency in some quarters to depreciate an argument which proceeds on chemical and physical lines is no refutation of the theory advanced.

Deformation of rocks by the shearing and crushing which have inevitably accompanied the great earth-movements, whose effect is traced in enormous foldings, fractures and overfolds, (by the resolution locally of normal into tangential forces in the contraction of the Earth's mass in the process of cooling) has certainly brought about petrological changes; but they do not appear to have resulted anywhere in the formation of a true schist or gneiss out of originally clastic rocks. This point is more fully discussed later on. Cotta pointed out in some remarks on the contorted strata about Vierwaldstädtersee (in a letter dated from Zürich in 1849) that the strata which we now see contorted must have been at the time in a more pasty condition from saturation with water, and probably have acquired their present rigidity by partial desiccation since their upheaval. Yet even in this pasty condition pressure has not succeeded in converting them into crystalline schists. No rocks on a large scale

* See Section v.

† The absurdity of this notion is exposed by Pfaff, (*op. cit.* p. 27.)

are equally saturated with water; and in great earth-movements it follows that with different degrees of plasticity (owing partly to this fact and partly to differences of constitution) it would necessarily result, that, while some portions were bent, contorted, and folded, other portions would suffer fracture, crushing, and in places even pulverization in different degrees. In such portions we should be prepared to find *clastic materials* present; but we have clearly no right whatever to infer from this fact the derivation of a deformed crystalline rock-mass from originally clastic materials. One of the main props of the theory of *extreme regional metamorphism* is thus knocked away.*

The simulation of the *macroscopic* characters of true schists by later rocks as the result, partly on the one hand of hydrothermal and chemical action, partly of deformation of rocks, by pressure, crushing, and shearing on the other, leaves open still a vast field for further microscopic research, united with field work.†

Among the accessory minerals common in the oldest crystalline rocks the most widely distributed are apatite, rutile, zirkon. The occurrence of these very stable minerals as accessories among the primal mineral constituents of the oldest rocks (granite, syenite, gneiss, mica-schists) as well as in the chief varieties of eruptive rocks, is entirely in accord with, and thus far lends support to, the theory that relative stability has been the main factor in determining (of course in the inverse order) the *general succession* of mineral deposits in the formation of the earth's crust.

It is not contended that in this there has been anything like absolute uniformity of succession. That we should consider on *à priori* grounds extremely unlikely, because we have no grounds whatever for assuming the thermal energy which pervaded the non-differentiated mass to have been equally distributed throughout it.‡ And so it happens—as theory would lead us to expect—that numerous irregularities are observed in the general order of succession; now this now that accessory mineral being common in the various types of the fundamental rocks; schists occurring subordinately among the gneisses, and gneiss occurring subordinately among the schists; garnet taking the place of mica, and giving us

* Since this was written a remarkable instance of the effect of crushing and subsequent decomposition of the pulverised materials in producing a *pseudo-breccia* in the gneissic rocks of the Malvern Hills has been described by Professor McKenny Hughes. (*Geological Magazine*, November, 1887).

† Kalkowsky (*Lithologie*, p. 43) remarks: "In Folge von Pressungen und Spannungen bei der Bildung können Körper dieser Klasse (einfach brechende Körper) doppelt licht-brechend sein, meist aber nur in schwachem Grade. Es bedarf eines sehr guten Polarizationsapparates und eines unermüdeten Auges, um solche Spuren von Doppelbrechung wahrzunehmen."

‡ See App. ii. Note R.

locally granulite in the place of granite; gneiss containing in different regions, some here others there, such accessories as graphite, garnet, tourmaline, epidote, rutile, zirkon, hornblende, chlorite, apatite, micaceous iron-oxide, magnetic iron oxide, pyrites; the mica contained in gneiss though generally muscovite, being sometimes biotite (Erzgebirge), as is the case occasionally with granite (Dartmoor); hornblende in one variety of gneiss, cordierite in another, augite in a third, garnet, graphite,* chlorite, green mica, all in their several turns, replacing, or appearing side by side with, the mica, and giving their respective varietal characters to the gneisses.

Yet through all this apparent confusion there is traceable a *leading principle in the oldest rocks*—general predominance of the more acid minerals (the most stable) in the granitoid and gneissic rocks, while, as we pass into the overlying schists and phyllites of the archæan series, the more acid minerals gradually give place more and more to the more basic minerals which are somewhat inferior (as a class) in stability to the minerals which preponderate in the granites and gneisses.

We have been here concerned (under the head of 'paramorphism') with a number of mineral changes, of which chemistry gives us some knowledge, among the materials which make up the mass of a rock *per se;* and we have not limited our view to changes which might occur after the rock was once formed, but have rather endeavoured—in a firm belief in the continuity of nature's principles of operation—to trace back the working of the principles learnt from physics, chemistry and mineralogy to the primal genesis of the most ancient crystalline rocks,—the granitoid, gneissic, and schistoid rocks of the earlier crust of the globe. In this we have arrived independently at some general conclusions which are found to be to some degree in accord with Gümbel's doctrine of 'diagenesis.' In all such changes we may regard the mass of the rock as remaining pretty constant in its composition after the primal accumulations of its materials, notwithstanding the changes which may take place reciprocally among its mineral constituents; so that a series of bulk-analyses would—could they have been made at the various stages of its history—have given pretty constant results. It is in this sense that one feels justified in the use of the term "paramorphism" adopted here.

Primary paramorphism has thus been treated as having to do mainly with the actual genesis of the rocks themselves through the agency of mineral change; and in this sense it comes within the larger subject of "metamorphism." We have been led to see strong reasons for regarding the processes

* See Appendix ii. Note L.

of primary paramorphism as having been determined by conditions which *must have prevailed universally and could only have prevailed universally*, at the surface of the globe at a very early stage of its developement out of its primordial nebulous mass.

The following laws and principles may be assumed as leading to this conclusion:

1. *The Law of Universal Attraction*, (the force of attraction varying directly as product of the masses, and inversely as the square of the distance); and the specialized operation of this law in all cases of *gravitation*.
2. Elevation of temperature when *latent heat* is set free either in the liquefaction of aëriform matter, or in the solidification of liquids.
3. Transformation of potential energy into kinetic energy, manifested as heat, in *chemical combinations*.
4. *Dissipation of Energy*, a tendency to equalization of energy throughout the universe when it is transformed into heat.
5. Transformation of energy of motion into heat in all cases of impact.
6. *The retardation of radiation* by such gases and vapours as are not diathermanous, with which the researches of Tyndall have principally made us familiar.
7. The enormous range of the condensation-temperatures of the several elements known to us, from that of Osmium or Ruthenium (App. ii, Note C) to that of Hydrogen gas, which our globe is far from having reached as yet.

These known and demonstrable laws and principles being assumed, it is not difficult to see how very high-temperature conditions of the Earth's lithosphere must have resulted at a certain stage of the evolution of the planet out of a nebulous mass, producing in fact the 'Solar Stage' of the developement of worlds. Recent researches in astronomical physics have so far verified the Kant-Laplace hypothesis as to warrant the assumption that the early history of the Earth has proceeded on such lines. Without attempting to follow Mr. Crookes* in his speculations on the genesis of the elements themselves, it is sufficient for our purpose to assume (as we may safely do) that, if this Earth has developed from a nebulous mass, the matter of which it is made up must have existed once in an extreme state of elemental dissociation.

Given then a nebulous mass of matter in such a state of elemental dissociation and losing heat by radiation into space, a point must be reached at which condensation of certain elements (those possessed of the highest condensation-temperatures, and the least potential energy of chemical affinity) must set in. As a direct result of this, concentration into a nucleus must follow from the law of universal attraction, as certainly as we see two corks run together when they float freely in water within such limits of distance that their mutual attraction is able to overcome the feeble cohesion of water. As the nucleus (the embryo-sphere) is thus formed, latent heat is set free, and the temperature of the nucleus is raised, giving off its heat by radiation, to be absorbed for the most part by the surrounding nebulous matter, and ultimately lost by radiation into space. As dissipation of energy progresses, further condensation must follow, the newly-condensed matter gravitating towards the nucleus, every increment of mass in this increasing proportionately the force of gravitation.

Equilibrium of pressure being upset in the mass by the resolution of the general law of universal attraction into the specialized force of gravitation, a certain rough stratification of the surrounding nebulous material must follow, at first perhaps resulting mainly in the condensation on a large scale of the vapours of the heavy metals, to form the outer zones of the growing

* See *Address* to the Chemical Section of the British Association, Birmingham Meeting, 1886.

'barysphere' (Suess), while the tendency of pressure to facilitate chemical combination (in other words to raise the dissociation-temperatures of oxides, haloids, &c.), in the inner strata of the nebulous material, must lead to a selective alteration of that material, tending in the direction of those conditions which have given us the residual atmosphere of later stages of the Earth's history. It would be easy to fall into the error of overestimating the pressure near the surface of the barysphere at this stage, if we overlooked the fact, that the mass of the barysphere must have been then less than that of the present globe; still there can be no doubt, from a consideration of the general conditions of the problem that the hydrostatic pressure was many times greater than that of our present atmosphere, though the value of the "g" of our ordinary dynamic formulae was then probably less than 32ft. per second. It may be predicated with a high degree of certainty, that as the nucleus grew into the barysphere, and every increment of mass increased the force of gravitation, *condensation must have gone on with great and accelerated rapidity;* with such rapidity, in fact, that the liberation of *latent heat* from the condensed matter alone would lead to a considerable elevation of temperature; and this would be immensely increased by the *adiathermaneity* of a very large proportion of the aëriform materials of the surrounding nebulous mass. Add to this *combustion on a grand scale*, as such elements as hydrogen, the alkali-metals, the metals of the alkaline earths, aluminium, magnesium, and at least the non-metallic elements, silicon, and phosphorus, became oxidized in the inner nebulous strata, their oxides, &c., being precipitated as further increments to the barysphere; and we may assert, as positively as any inferential truth can be asserted, that with such an enormous and rapid developement of heat at and near the surface of the barysphere and in the rapidly-growing lithosphere, together with such retardation of radiation into space as must have been caused by the nature of the dense nebulous envelope which surrounded it, *the developement of thermal energy qud heat must have been for a considerable period of the Earth's history much more rapid than its dissipation by radiation into space.* Here then we have all the factors needed for producing the high-temperature conditions required by the theory of the genesis of the archæan rocks which has been put forward in this work.

The retardation of radiation at this stage by the nebulous envelope will be seen to be very great, if we consider only the fact that owing to (*a*) the high-temperature conditions of the surface of the barysphere as the lithosphere began to form upon it, and (*b*) the enormously high temperature at which steam undergoes complete dissociation into elemental hydrogen and oxygen, most of the water now existing on the globe and shut up within the lithosphere, must have *then existed as aqueous vapour in the outer nebulous envelope (the primordial atmosphere).* The effect of this in retarding radiation has been rendered perfectly clear by the researches of Tyndall and others on radiant heat; and in the lithosphere-forming stage its effect differed only quantitatively from what we observe on a cloudy summer evening. Again, it will be seen that the action of the non-diathermaneity of the primordial nebulous atmosphere in shutting off a large portion of the solar rays does not materially affect the argument; for this is quite independent of any such accession of heat from sources outside the Earth's mass. And this consideration is an essential one, since recent researches in astronomical physics* show that we cannot safely assume that the central orb of our present Solar System had at that time reached its present 'solar' phase.

Nor are we to imagine that deposition by condensation which was the main factor in the formation of the lithosphere (at least in its earlier stages) went on over the whole surface with geometrical regularity. On this point it will suffice to quote from the concluding paragraph of Suess's 'Entstehung der Alpen': "Tacchini has shown that irregular areas are developed on the solar disk which give only the magnesium-spectrum, and that these vanish again.

* See especially Prof. S. P. Langley, *The New Astronomy*, (Boston, 1888).

Zöllner and before him Bullialdus have attributed the periodically-variable strength of the light of certain stars of variable brilliancy to the presence of dark fields of slag and of glowing 'Meers' upon the surface of their rotating spheres during the process of cooling. The condition of our planet may well have been once similar." But this carries us a step further, when, by the continued operation of the same laws, not only did the intensity of heat reach a maximum, followed by another phase of considerable duration, in which developement of heat in the first-formed lithosphere and dissipation by radiation (as the nebulous envelope resolved itself, owing to progressive condensation, gradually into an atmosphere) were practically balanced (the 'solar phase'); and this again (with further advance towards atmospheric conditions) was followed by an inversion of the relation that previously subsisted between developement of heat and its dissipation by radiation, *the latter now getting the mastery*,—the cooling stage, through which our planet is still passing, having set in.

There are reasons for believing that the larger mass of Jupiter is at present in a much earlier phase of the cooling-stage than our Earth.

β. **Secondary Paramorphism.** Rocks having been once formed there is no cessation of the action of chemical and physical laws, though the *conditions* under which they continue to act are changed by dissipation of energy. We must therefore be prepared to recognise a progressive order of changes among the minerals of the same rocks, especially by the action of percolating and interstitial water, and to some extent, where within reach of the atmospheric waters of the globe, by the action of carbonic acid and other atmospheric gases dissolved in rain-water, as well as by the potent action of the humus acids, which result from the decay of vegetation (under conditions of incomplete oxidation), especially in the dense jungle-growths of tropical regions.*

Such agencies lead in many cases to the formation of a series of minerals which were not formed in the original genesis of the rock, and are therefore conveniently spoken of as 'secondary minerals.' Further, inasmuch as the formation of such secondary minerals—formed as they are in the main by derivation from the original and primary minerals of the rock—in no way (without concomitant hyperphoric change) alters the composition of the rock as shown by a bulk-analysis, we may include all such changes under the head of 'secondary paramorphism.' This is exemplified in a great variety of rocks, some instances of which may be here cited.

a. In the formation of secondary minerals in phyllites or slates, and in shales, as separation-products from the original materials of the rock (see Sect. iv.).

b. In the separation-out of the siliceous cementing-material of altered and compacted volcanic tuffs.

c. In the decomposition of the felspar contained in some varieties of sand, to furnish the siliceous cement (with its

* Strikingly exemplified in the formation of *laterite*, as an extreme product. (See Appendix i.)

inclosed kaolin) of some quartzites. The case of the genesis of the Sarsen-stones, which I have attempted elsewhere to explain,* may be mentioned here. The theory which I put forward five years ago has since been quoted by other writers, and I have met with no attempt to refute it. The theory itself was worked out on purely chemical lines and confirmed by microscopic examination of thin sections.

d. In the deposit of calcite in igneous crystalline rocks by the action of carbonated waters upon felspars containing lime to which I drew attention in the paper just referred to.†

e. In the formation of secondary quartz out of the matrix of acid rocks, such as the quartz-porphyries of the Bozen district.

On general grounds there is no valid reason why in acid rocks secondary SiO_2 should not be formed as freely as more basic minerals (chlorite, serpentine, etc.) are formed in highly basic rocks.

No one can study Rammelsberg's 'Mineralchemie' without being impressed with the very large proportion of ordinary rock-forming minerals which are not in the strict sense of the word pure chemical compounds; that is to say compounds in which the metallic (basic) and non-metallic (acid) constituents are present in exact equivalent proportions. In many cases of minerals formed by dry heat (not a trace of water being contained in them), as the primordial minerals must have been, the excess of one or the other class may have promoted 'fluxing,' so that the minerals have assumed a definite individuality on crystallizing from dry fusion; but it by no means follows that in such cases the most (chemically) stable compounds have crystallized-out. There must remain in them therefore an innate tendency to reach the more *stable condition of chemical equivalency* (especially when aided later by the access to them of superheated H_2O), and this could hardly take place without the separation-out of the excess present (beyond the equivalent proportions) of the basic or acid constituents as the case might be. Such paramorphic change must clearly be distinguished from alterations resulting from chemical reactions of foreign mineral-matters in aqueous solutions.

The origin of some of the quartz in quartz-porphyries still admits, I think, of discussion (see Zirkel *Besch. der Min. u. Gest.* pp. 330, 331), especially that which contain portions of the felsitic basis.

f. Judd‡ has recently recorded the presence of many secondary minerals as alteration products in the Tertiary igneous rocks of the north-western portion of the British area. That author recognises three agencies as brought into play in this process of secondary paramorphism:

(i.) *Solvents acting under pressure*, producing that class of change which he distinguishes as 'schillerization.'

* See Proc. Geol. Assoc. vol. viii. pp. 153-160; also Brit. Ass. Report (Southport, 1883), p. 505.

† *Ibid*, p. 158.

‡ Q. J. G. S., vol. xlii, pp. 80-86.

(ii.) *Steam and other gases* acting upon lavas at the surface. Among the results of this kind of action he mentions palagonite, amygdaloidal in-fillings of steam-cavities with such derived minerals as zeolites, chlorophæite, epidote, quartz, calcite; while secondary quartz and calcite are in some cases "found to have been introduced into the substance of the rock."

(iii.) *Atmospheric agents* producing kaolinization of the felspars accompanied by hydration and recrystallization of the silicate of alumina, and in some cases concomitantly with the breaking up of the felspar "the separation of considerable quantities of silica which crystallizes as secondary quartz." Varietal changes in the augites, paramorphic conversion of augite into hornblende, conversion of augite into viridite and this eventually into hornblende, conversion of olivine into serpentine, are among the most interesting examples of secondary paramorphism noted as occurring in those particular rocks.

Special interest attaches to the recent valuable work of Professor Judd, which has so greatly enriched the Quarterly Journal of the Geological Society. I do not think however that the idea of 'petrological provinces' (which he has recently formulated) is altogether new to science; for it seems to underlie much that has been written by continental writers and to be tacitly implied as a limitation of the views of some of them as to a temporal order of succession of the eruptive rocks. In a descriptive classification of the crystalline rocks we must take them *as they are presented to us in nature*; and (the importance of time as a factor in all the complicated mineral changes involved, together with the importance of high temperature and pressure to increase the solvent action of water being admitted in the more extreme changes) common sense would lead us to expect to find generally those eruptive rocks, in which paramorphic change has taken place in the more extreme degrees, rather among the palæozoic than among the latter rocks.*

g. Among the workers in this country whose researches have thrown great light on the mineral changes (secondary paramorphism) which take place in crystalline (igneous) rocks, a very high place must be assigned to Allport, not merely for the value of his papers, but as a recognition also in him of a pioneer.

In his classical paper on the Rocks of the Cornish Peninsula (Q.J.G.S., vol. xxxii) he says of the 'Altered Dolerites' that "microscopic examination

* cf. Bonney's Presidential Address, 1886.

clearly shows that the pyroxenic minerals (augite and diallage) have frequently been converted into a hornblendic substance, and the variety enstatite is found filling cavities and fissures precisely in the same manner as other products of alteration." (p. 424). "The alteration that has taken place appears to be the result of internal (paramorphic) rather than of external action—decomposition and rearrangement (a vague term, requiring considerable definition from the chemist's point of view) of mineral substances *in situ*—and not to any great extent to the introduction of new minerals from without."

On p. 425 occurs the inferential statement:

"Two rocks of similar origin and composition may follow different lines of metamorphism according to the nature of the active agent and the duration of its operation." As examples he points to the following:

(1) The Clickor Tor dolerite at a distance from the granite having been altered into a serpentinous rock by aqueous agencies, the serpentine forming pseudomorphs after olivine, and the rock having been originally an olivine-dolerite;

(2) The Penzance dolerites in close proximity to the granite having been transformed into hornblendic rocks, varying *(inter se)* in texture and extent of alteration according to the coarseness or fineness of the original crystallization.

In the same paper it is shown that "many of the metamorphic rocks described have undergone a second series of changes, as indicated by the occurrence of micaceous and chloritic pseudomorphs after tourmaline, and an alteration of the mica;" that "the granite has also suffered similar changes;" and that among the more basic rocks hornblende-schists may be metamorphosed igneous rocks, some being derived from dolerites and gabbros (implying both secondary paramorphism and metataxis), while others are very probably foliated diorites (implying metataxis only).

In his great paper on British Carboniferous Dolerites (Q.J.G.S., vol. xxx) the same author has shown how great and varied are the changes which these have undergone, including such as

(1) Devitrification of an original glassy ground-mass.
(2) Partial kaolinization of felspars.
(3) Formation of magnetite grains within augite-crystals.
(4) Partial conversion of augite into 'a grey fibrous or granular substance.'
(5) Conversion of augite into 'a fibrous hornblende.'
(6) Alteration of olivine into a green substance, generally serpentine chlorite, chlorophaeite, or ferrous silicate.
(7) Further alteration of serpentine into haematite.
(8) Pseudomorphs after olivine composed of calcite and serpentine and other minerals.
(9) Aggregations of grains of augite in felspar.

Allport suggests (p. 540) the "long-continued action of mineral-waters" (*i.e.* waters holding mineral salts in solution) and circulating in the interior of the 'crust' as the cause of the changes which occur in olivine.

A comparison of these changes with those described by Judd in the Tertiary igneous rocks and referred to, (*supra*, p. 26,) brings out some differences in the action of water at or near the surface and at depths in the crust.

The differences observable in parts of the same rock in the same quarry (partly from unequal admixture of the original minerals, and partly from different degrees of paramorphic change which they have suffered) are seen to be as great as those by which such rocks as dolerite, basalt, anamesite, aphanite, are distinguished; so that there would seem to be little reason why all the basic augitic rocks should not (as Allport argues) be included in the one group of dolerites.

In subsequent papers, (1) 'On the Ancient devitrified Pitchstones and Perlites from the Lower Silurian District of Shropshire' (Q.J.G.S., vol. xxxiii) in which he compared the changes which these have undergone (as exhibited in the Lea Rock of the Wrekin, which I had the pleasure of examining a few years ago in the company of Dr. Callaway) with the changes exhibited by the

later pitchstones of Meissen and perlites from Kremnitz and Schemnitz; (2) 'On the Diorites of the Warwickshire Coalfield' (Q.J.G.S., vol. xxxv), Allport has shown how little essential difference there is in rocks of different ages of these two several classes or groups, after we have eliminated what would be included (in the nomenclature of this work) under the heads of (a) metataxic, (*b*) hyperphoric change, the latter being determined mainly by two circumstances in the environment of the rocks, (i) the nature of the adjacent rocks, (ii) extent and time of exposure to the atmosphere (chiefly resulting in the formation of haematite by the introduction of free oxygen, and calcite by the introduction of CO_2 held in solution in atmospheric waters), amounting in fact to what is commonly called 'weathering.' Leaving therefore these minor and localized alterations out of account, may we not generalize from Allport's work so far as to regard the really paramorphic changes which the igneous rocks exhibit as consisting in the aggregate (from the chemical point of view) in the mineral-transformation from those states of combination which were most stable in a condition of *dry fusion* to those which are most stable *at lower temperatures in the presence of water*?

To Allport seems then to belong the great merit of having laid the foundation of a *natural system of classification* of the igneous rocks. That such a system, on such principles as are illustrated in the outline suggested in Bonney's Presidential Address (1885), must ultimately supersede more artificial systems (as in Botany the 'Natural Orders' of De Candolle have superseded those of Linnæus) seems hardly doubtful.

The masterly researches of Allport *(infra)* followed up by those of Judd, Bonney and others, have made out such a strong case for the genetic identity of the Palaeozoic and Tertiary rocks of each class or family, that it is with great satisfaction that one finds so high an authority as Credner (*Geologie*, 6th ed. 1887, p. 303), after doing full justice to the systems of Zirkel and Rosenbusch and drawing a parallel between the several types of the "paläovulkanische und neovulkanische Gesteine," admitting that "zwischen jenen alteren Eruptivgesteinen und den vulkanischen Producten der Jetzzeit ist mit Bezug auf ihre Entstehungsweise *nur ein zeitlicher Unterschied.*"

What is an individual rock? This question must be answered with reference to architectonic *functions* displayed rather than to composition. Here we look for the great broad class-characters. But when we come to break up a class (such as *(a)* massive crystalline rocks, *(b)* phyllolithic crystalline rocks, *(c)* clastic rocks,) into sub-classes, or groups, or families, we must rely upon characters which are subordinate to those which have determined our classes. It is here that the *quaestio vexata* crops up. Are we (following Rosenbusch) to make "mode of occurrence in the field" the main basis of our sub-classes and orders or families (which is rather suggestive of the adoption of a grouping into forest trees, shrubs and herbs, as a basis for classification within the limits of either of the great classes Exogens, Endogens, or Acrogens); or, are we (as Bonney suggests) to give the first place to the broad differences of mineral composition (which is more analogous to the grouping of *e.g.*, the Dicotyledons into sub-classes by floral structure, as is usually done in the natural system)?

Look at it how you will, there are considerable difficulties for the igneous rocks in considering anything less than the *entire crystalline mineral complex*, which is the product of any particular centre of igneous activity, as possessing definite *individuality*. This is well shown by the well-worked-out 'Eruptiv-stock' of Predazzo, as sections of it are exposed by erosion in the flanks of the deep gorges of the Avisio and the Travignolo, (Credner, *op. cit.*, p. 587). Here tourmaline-granite, syenite, melaphyre, augite-porphyry, orthoclase-porphyry, are all found with their respective morphological modifications—some occurring as plutonic (Tiefengesteine), others as dykes (Ganggesteine,) others again as out-poured lavas, (Ergussgesteine)—yet with no more definite individuality than can be assigned severally to the root, the stem, and the foliage of an individual tree. We should scarcely attach any serious value to

a classification based on the morphological characters of separate collections respectively of chips or fragments of roots, stems, and leaves of plants. It is the way in which their structural characters are blended together in the individual plant that serves as the starting point in the natural system of classification in the Vegetable kingdom. Such an extension of the term 'individual' would absorb the troublesome factor of 'temporal succession,' and would do no violence to the idea of 'petrological provinces.'

h. In the case of interbedded eruptive rocks which have been formed as sub-marine lava-flows, it is clear that the secondary paramorphic changes which they must have undergone in the process of cooling will be somewhat different from those which have taken place in sub-aërial lava-flows. In the former case the salts contained in solution in marine waters would set up a rather different set of reactions within the rock mass. The salts of the alkali-metals contained in such waters would perhaps not undergo much change by contact with the minerals contained in the original lava-flows, since both their acids and bases are electro-chemically stronger than those contained for the most part in the silicates. In the comparatively rare instances in which reactions were set up between them (*e.g.* in the reaction between silicate of potash contained in a felspar or mica and sodium chloride) the K Cl thus formed would be removed in solution, while the nature of the felspar would be altered by the substitution of sodium for potassium; and in this way it is conceivable that in many sub-marine lava-flows orthoclase may have been converted into albite.* In the case of the salts (the haloids, the sulphates, chiefly) of lime and magnesia we should expect changes of this kind to occur more extensively. These would all react upon silicates of the alkalies to form corresponding soluble salts of those metals, the Ca and the Mg replacing the Na or the K to form non-soluble silicates. It is impossible therefore to deny that *serpentinization* and other essentially similar changes may have taken place in this way, or that orthoclase and albite may have been converted by such a process of sub-marine paramorphism into oligoclase, anorthite, labradorite, or saussurite.

The extent to which water in such cases might penetrate the glowing molten mass would vary with the pressure as determined by depth below the surface; and perhaps in any case steam only would penetrate the mass under such conditions. But there can hardly be any doubt that as the mass cooled it would be penetrated by saline water. The idea seems worthy of consideration in working out the history of such rocks as Troktolites and Eucrites, and generally of those rocks in which anorthite occurs. See (*e.g.*) Teall (Q.J.G.S., vol. xl, pp. 234-235.) The two chief varietal characters noted by Mr. Teall in

* For a probable instance of this in a dyke penetrating the strata at the bed of a Cambrian sea, see Q.J.G.S., vol. xliv, p. 410.

the well-known Tynemouth Dyke, are (1) porphyritic crystals of anorthite ('very irregular, rather crystalline aggregates than simple crystals'), (2) small spherical amygdaloids of calcite. The very unequal distribution of these in the rock, and its position in the Carboniferous rocks beneath the magnesian limestone, point to the action of infiltrating water of the sea which yielded the magnesian limestone strata.

May not the richness in magnesian-silicates of some altered and compacted diabase-tuffs be possibly accounted for in some instances by the infiltration of marine waters where the volcanic ejecta were spread out on a submarine floor? (See Kalkowsky *Lithologie*, p. 120).

From this point of view especial interest seems to me to attach to the exhaustive study Judd has made of the 'Tertiary and Older Peridotites of Scotland' (Q.J.G.S., vol. xli, pp. 354 *et seq.*). Most instructive are the alterations noted in the felspars (plate x); and the occurrence of sulphates and chlorides in the liquid enclosures of some of these (*op. cit.*, p. 376) is extremely suggestive of the action of infiltrating saline waters. On p. 377 I find that Judd notes "*serpentinous* and other decomposition products" as results of the "passage through crystals of felspar of water from the surface." Kaolinization is certainly the general result of the action of carbonated atmospheric waters; but in all such cases we have only the action of a *free acid* (CO_2). In saline waters (whether furnished from the sea or from underground circulation of mineral-waters) the case is quite different: the several *bases*, with which the acids are combined in the dissolved salts, must be accounted for; and from the chemical standpoint we can only account for them as has been suggested (by the formation of silicates with bases of lower chemical energy), unless they remain mechanically mixed with the quartz which results from the SiO_2 replaced by the stronger acid. (See App. ii, Notes S.) On general grounds the latter would appear very unlikely in cases of the kind here discussed.

We are justified from such considerations in declining to accept the evidence of microscopic and chemical analysis as of itself conclusive, without reference to evidence in the field as to the history of such rocks. By similar reactions we can understand conversion of potash-micas (muscovite and damourite) into soda-mica (paragonite) by the action of salts (chiefly the haloids) of sodium; and the ultimate production of magnesia-mica (containing in some cases more than 30 per cent. of MgO), the iron-micas, and even baryta- and lime-mica (margarite). For data for these applications of chemical principles see Rammelsberg.* The same high authority tells us (p. 510) that "viele Glimmer sind offenbar Umwandlungsproducte älterer Silicate und deshalb leicht Gemenge."†

i. An interesting case has recently been described of alleged regional metamorphism of vast areas of sedimentary rocks of Cretaceous age in America (see *Nature*, May 27th, 1886). It is not at all impossible that the chemical agencies, which have been during all time and are still at work, have been able to

* *Mineralchemie.*, pp. 510-537. See also Roth's *Allgem. u. Ch. Geol.* Bd. I., p. 68.

† An instance of the derivation of mica from felspar has recently been described by Bonney (Q.J.G.S., vol. xliv., p. 36, fig. 2); and Kalkowsky (*op. cit.*, p. 208) remarks that the felspars produce by their decomposition kaolin and micaceous minerals (glimmerartige Mineralien.)

produce in some regions a certain degree of paramorphic change, which is however quite distinguishable with modern methods of research from the characteristic crystalline structure of the archæan rocks. The derivative relation of these clastic rocks to the granite in the Californian region is similar to that of the Pläner Sandstein of the Dresden country and Saxon Switzerland to the syenitic *massif* of that region.

On many points connected with the Californian region described by Mr. Becker, we must for some time suspend our judgment.

The prevalence of *zoisite* may very likely point to an extensive conversion of the carbonate of lime (which forms the chief cementing material of these clastic rocks) directly into silicate of lime, by the replacement at high temperature of its CO_2 by the SiO_2 of the quartz; it being an experimental fact well known in the chemical laboratory that crystalline quartz in a state of fine division can thus form silicates of the alkalies by simple contact with their carbonates in a state of fusion at ordinary atmospheric pressure; and so probably can form silicates of the alkaline earths* in a state of dry fusion, such as that in which statuary marble must have once existed. The silicate of lime may have served as the basis from which other minerals have been formed, and so the peculiar "pincushion" arrangement of needle-like crystals stuck in the quartz-grains may be partly accounted for.

So far as the evidence at present to hand seems to carry us, it looks as if the case cited here were one of slight secondary paramorphism extending through an extensive region. To assume however that this is but an incipient stage of a metamorphic process, whereby the rocks of this region may ultimately be converted into crystalline schists or gneiss is altogether unwarrantable.†

k. Of the "resolution of clastic grains into crystalline aggregates" with the simultaneous conversion of sandstone

* This deduction has been experimentally verified (see App. ii. Note A.)

† Mr. Becker has dealt with the rocks of this region in his Essay which appears in the '*Etudes sur les Schistes Crystallins*' published by the International Geological Congress, 1888; but it can scarcely be said that much new light has been thrown upon the subject. The crucial point of his argument is the alleged transition from the great crystalline series (which have been apparently and to a subordinate degree rendered schistose along crush-planes) covering 3000 square miles and more into the clastic series, which have undergone excessive crushing. Such alleged transitions have been disproved over and over again in Europe, and we want more precise information as to the nature of the crushing and welding that has occurred at their junction.

Extensive secondary developement of crystalline minerals in fissures, even in a rock which has been in part 'reduced to a mere rubble,' with an elevation of temperature corresponding to the intensity of the crushing forces, can scarcely be said to have produced a *crystalline schist*, when the 'unfissured masses of considerable size never show any notable alteration.' (p. 111.)

into quartzite I have myself observed a local instance in the metatropic alteration of the basement-beds of the Keuper sandstones of Grinshill, Salop, where they are faulted (as seen in the lane leading from that place to Clive) at a high angle against the Bunter Sandstone. The Bunter Sandstone is altered against the fault into a sort of brick, and the alteration of the Keuper Sandstone into a quartzite extends from twelve to fourteen feet into the Keuper beds, beyond which these assume their normal character. There is a developement of quite large macroscopic crystals of a flesh-coloured silicate along certain zones of the rock near the fault. The phenomena presented in this faulted section seem to me to afford a very good example of the effects of the intensity of heat when suddenly developed by pressure and sliding friction sufficiently concentrated, as in some of the late Mr. Mallet's experiments. It seems reasonable to regard such occurrences as concomitant subterranean phenomena of great earthquakes, and as being no more related to such so-called 'regional metamorphism' as is seen in the 'crystalline schists' of the archæan rocks than a sprat is to an elephant or a whale. (See App. ii.)

In nature no limit can be recognized in the operation of those changes to which we have assigned the name 'Secondary Paramorphism,' in effecting which *water is the chief agency*. They are seen in operation not only in rocks commonly called 'metamorphic,' but in igneous (plutonic and volcanic) and sedimentary rocks also. Secondary quartz crystals (as was shown by the late J. A. Phillips, and cases of which I have myself recorded*) are developed on sand-grains; quartz crystals and intergrowths of crystals of quarts and felspar are found developed on the surfaces of blocks of felspathic rocks in some of the older conglomerates, as in those in Euba near Chemnitz quoted by Credner†; crystalline granular aggregates of quartz, orthoclase, oligoclase, mica, and tourmaline are found deposited by mineral waters in fissures; veins are formed in granitoid rocks by the infilling of fissures with secondary minerals dissolved out of the rock-mass, as in the granulitic rocks of Saxony, in the Riesengebirge, in the Isle of Elba, in the gneiss of North America (Credner, *op. cit.* and the authors there cited.) Deposition of minerals in the drusy cavities of rocks, from which those minerals have been derived, are further examples of this, one of the most universal of nature's operations.‡

* Geol. Mag., dec. ii, vol. X., p. 412.

† 'Elemente der Geologie,' 3rd ed., p. 203.

‡ To these we may add the cases of secondary change recently described by Bonney (Q.J.G.S., February, 1888,) (i) in the matrix of the Obermittweida conglomerate, (ii) in the matrix of the conglomerate of Sudbury, Canada.

To sum up, in the words of the writer just quoted,—"The tendency of water universally is either to dissolve the mineral-constituents of a rock directly, or, after decomposition of insoluble compounds, to remove at least some portion of them in solution."*

Of the changes considered here under the head of 'Secondary Paramorphism' it may be said generally, that they are local, partial, and accidental; they result in giving *varietal differences* to different portions of great rock masses, but they are in no way essentially connected with those characters which give to any great rock-mass *a definite individuality, as performing a determined function in the developement of the earth's lithosphere.* As Judd cogently remarks, (Q.J.G.S., vol. xli, p. 362), "much confusion has been introduced into petrographical literature in consequence of *all the characters* presented by minerals being treated as if they had precisely the same [degree of] significance. While some of the characters of the rock-forming minerals are original and essential, others are, as certainly, *secondary and accidental.* The minerals, since their first crystallization, may have undergone several series of changes totally dissimilar in kind, and resulting from causes altogether different." The italics in this quotation are mine.

§ iii. METATROPY.

In using this term, we must give ourselves rather more latitude than is allowed in the use of the sister-word 'allotropy' in chemistry. Strictly, allotropy implies the assumption under different physical conditions of a *different set of physical properties by one and the same chemical body*; and its use is generally confined to the elementary bodies. Here we shall have to preserve the main idea, and, while extending the term metatropy to chemical compounds, and even to mixtures of them in some instances, include under it only those instances in which essential alteration takes place in the physical character (τροπος) of the rock (hardness, crystalline form, cleavage, fracture, optical properties, conductivity, specific gravity, and so on), while no *essential* change occurs in its chemical composition. Such changes, it will be seen, may be considered without reference to the original genesis of the rock. The conversion of seam-coal into anthracite (as in the Culm of the Alps and Germany), and that in some cases into graphite, are changes in the physical character of rock-masses which may be designated 'metatropic,' concomitant with other changes in the rocks among which they are interstratified. In this case we have a slight change in chemical composition, with a corresponding increase in the percentage of the elementary carbon, but the chemical composition remains essentially the same.

* Roth has discussed the whole thing in his usually masterly way in his *Allgemeine und Chemische Geologie.* To his laborious collection of facts as chemical data for the study of such phenomena, references are made in the sequel of this work.

The formation of palagonite and hydrotachylite out of the glass of basalts by hydration, the change of anhydrite into gypsum by taking up water of crystallization, may perhaps be considered cases of metatropy, as also the conversion of arragonite into calcite by dry heat, and the opposite change of calcite into arragonite by solution and reprecipitation at a higher temperature. Such instances of 'contact metamorphism' as the conversion by heat of the finer grauwackes and shales into hornstone or porcellanite, the conversion of coal into coke, the conversion of sandstone into quartzite, and the conversion of limestone into marble, are all instances of metatropy. Occasionally there would appear to be a slight and subordinate *metataxic* change also, resulting in the appearance of small inclusions of earthy or marly matter in crystalline marble, unequal distribution of the oxides of the heavy metals, a linear arrangement of authigenous mica in cipollino (as is well seen in the weathered columns of the Roman Forum) the cleavage of some Alpine marbles. In some of these changes heat has been the sole or principal agent; but in the case of marble pressure also was absolutely essential to prevent the destruction of the carbonate of lime by dissociation into calcium oxide and carbonic acid gas.

Polymorphism, a phenomenon exhibited by many minerals, often to the extent of rock-masses of considerable magnitude, may be included under the head of metatropy. Here again the study of the mineral constituents of rocks on the chemical side seems to help us, as the facts next to be cited plainly show.

(1) *Temperature* may affect the crystalline form of the mineral, as illustrated in the dimorphism of carbonate of lime, to which reference has been made before.

(2) The crystalline form of the same essential compound may vary with the amount of *molecular water* which its molecule holds in combination, as in the case of carbonate of magnesia, which crystallizes out of an aqueous solution of the salt in carbonated water, when the solution is left to stand in the air, in the following forms:

(a) in monoclinic plates with the composition $Mg\,CO_3 + 5\,H_2O$ in the cold of winter;

(b) in a mixture of monoclinic prisms with the composition $Mg\,CO_3 + 4\,H_2O$ and nests and balls of rhombic twinned crystals of the composition $Mg\,CO_3 + 3\,H_2O$ at ordinary temperature;

(c) as a fine powdery precipitate of the composition $Mg\,CO_3 + H_2O$ from tepid water.*

* Wislicenus, *Anorg. Chem.*, § 600.

With the case of the variation of molecular structure along with differences in the proportions of molecular water presented by magnesium carbonate, as observed in the chemical laboratory, may be mentioned the case of the native hydrates of alumina:

Hydrargillite, $Al_2O_3+3\ H_2O$ [$H_6\ Al_2\ O_6$] chalcedonic.

Bauxite, $Al_2\ O_3+2\ H_2O$ [$H_4\ Al_2O_5$], earthy.

Diaspore, $Al_2\ O_3+H_2\ O$ [$H_2\ Al_2\ O_4$], highly crystalline (isomorphous with chrysoberyll ($Be\ Al_2\ O_4$).

The most crystalline form here, it will be noted, has least water, while in the $Mg\ CO_3$ series it has the highest proportion of water.

In the silicates of copper again we have

Dioptase, $Cu\ Si\ O_3+\ H_2\ O$, crystalline:

Kieselmalachit, $CuSi\ O_3+2H_2O$, non-crystalline.

(3) *The presence of an accessory mineral may influence crystalline form.* In the crystallization of carbonate of lime the presence of small quantities of the carbonates of barium and strontium (for example) increases greatly the proportion of *arragonite*, with which those minerals are *isomorphous*; a fact which would suggest that the normal crystalline form of the carbonates of the alkaline earths is rather that of the rhombic prisms of arragonite than the hexagonal system, to which the many crystalline forms of calcite may be referred.

Recent researches seem to suggest an allotropic relation between certain augites and certain hornblendes. Within the wide range of variation which these minerals severally display in their chemical composition, it is not at all unlikely that for certain percentage-compositions, the same mixture may crystallize under certain physical conditions as augite, and under other physical conditions as hornblende. But have we any right to generalise to the extent of asserting that any augite may undergo a metatropic change into hornblende?

A negative answer to this question is suggested by the following average percentages:

	Augite (29 analyses).	Hornblende (24 analyses).
$Si\ O_2$	49·70	44·11
$Al_2\ O_3$	5·78	10·86
$Fe\ O$	8·58	7·61
$Fe_2\ O_3$	·85	6·13
$Ca\ O$	20·71	12·35
$Na_2\ O$	·19	1·67
$K_2\ O$	·07	1·41
MgO	9·95	14·20

from analyses given by Rammelsberg.

Rammelsberg (*op. cit.* pp. 411, 414) regards both minerals as "isomorphous mixtures of silicates" with the sequi-oxides Fe_2O_3 and Al_2O_3, though his earlier notion that the $Al_2\ O_3$ was present in hornblende as *aluminate*, may not be altogether untrue; and, if so, this would possibly help to determine the crystalline form of hornblende. More importance perhaps is to be attached to the frequent occurrence in the aluminous hornblendes of such accessory components as TiO_2, CrO_3, and F, as influencing crystalline form. (*Ibid*, pp. 416-418.).

Again, Epidote and Zoisite are almost allotropic forms.

In this connection may be mentioned those curious globular concretionary structures with a radiating prismatic sub-crystalline texture—suggestive to a chemist's mind of crystal-

lization out of supersaturated solutions—which abound in the Magnesian Limestones of Durham, occurring sometimes nearly a foot in diameter, while smaller ones often make up the entire rock-mass of whole beds of limestone, containing 98 per cent. of carbonate of lime.*

Of all the changes which we should call metatropic those of *vitrification* and *devitrification* of rock (as, *e.g.*, obsidian and tachylite) are by far the most complicated. It is pretty generally recognized that the former of these is the result of solidification by rapid cooling. Much obscurity however still hangs over the latter phenomenon; but perhaps the consideration of a few known chemical facts may go a little way towards penetrating it, and will lead us to recognise a *change of molecular structure* as the essential part of the process of devitrification; though the physicist and the chemist pronounce with one voice our ignorance of the actual constitution of the molecules of solid bodies. Two of the best-known instances of allotropy are those of the elements *sulphur and phosphorus*.†

Both these bodies may undergo such a metatropic change as to assume an allotropic modification, which we are justified in calling the 'vitreous condition.' I have for some years used this term with reference to them, and observe that this use of it is gradually finding its way into text-books of chemistry. In both cases the vitreous condition is induced by rapid cooling; common or vitreous phosphorus, by the arrangement employed for casting it into sticks; sulphur, when poured in a molten state at temperatures not far below its boiling point into cold water. The translucent flexible needle-shaped prisms which are formed when sulphur solidifies in the dry way at the higher temperature of its fusing point (115°C) must be regarded as a vitreous form, so far as their internal texture goes, since they are quite isotropic.‡ That in the vitreous condition of both these bodies the molecular structure is *not the most permanent or stable* appears from several considerations. Vitreous sulphur (both in the plastic and in the prismatic state) is known to assume the crystalline state in forms of the ortho-rhombic system in a few days, the translucent mass becoming opaque (devitrified) as the result of crystallization.

* They must not be confounded with concretionary dolomitic 'mudstones' found in other parts of the same rocks, on the weathered surface of which a laminated structure can be easily traced. In the summer of 1886, to the surprise of Mr. Howse, of the Newcastle Museum, I split some of these with the hammer, and disclosed casts of *Axinus* within them.

† Carbon, another allotropic element, is omitted here, as it is not known in the glassy or isotropic modification; but we do know that in its crystalline state (Diamond, Sp. Gr. 3·5) its density is greater than in the amorphous state (Charcoal, Sp. Gr. 1·82) (*cf.* Note L, App. ii.).

‡ They are in fact crystallites.

Further there is found both in flowers of sulphur and in vitreous sulphur an amorphous electro-positive form of the element (insoluble in CS_2 and other solvents in which normal crystalline sulphur readily dissolves), and this amorphous sulphur is found in greater proportion as the sulphur has been more strongly and continuously heated and more suddenly cooled. This may be well compared with the amorphous 'dirt' which the microscope reveals in the vitreous forms of volcanic rocks; so that this may perhaps be regarded as a *concomitant product of vitrification.* Amorphous insoluble sulphur is also formed on the surface of the mass when molten sulphur is allowed to congeal in the presence of strong sunlight, just as vitreous phosphorus is partly converted (with the assumption of various shades of red) into amorphous phosphorus by the same physical agent. Prolonged action of heat too, as is well known, converts the whole of a mass of vitreous phosphorus in the red amorphous variety; while the same result is effected by heat in much less time, if a trace of iodine or phosphoric iodide is present. Amorphous sulphur however ultimately assumes the normal crystalline form, as does also vitreous sulphur; and the change is accompanied in both cases by the liberation of heat, the sudden crystallization of vitreous (plastic) sulphur being accompanied by a sudden rise of temperature from 93°C to 110°C, when the experiment is properly conducted. (See App. i, *a.*)

We may say then with Prof. Wislicenus* that "the orthorhombic modification of sulphur is at ordinary temperatures the most stable, into which all others pass spontaneously; native sulphur occurs accordingly without exception in this form." Selenium undergoes a similar series of metatropic changes. Less is known about the crystallization of phosphorus, but vitreous phosphorus is well known to form a crystalline sublimate, when kept for some time in the dark in an exhausted and hermetically-sealed glass tube. In my own work too I have obtained evidence of the spontaneous transformation of the other allotropic forms of phosphorus into a crystalline sublimate. We may then regard the *crystalline* condition of both sulphur and phosphorus as the *most stable*, as that towards which there is a constant *strain or struggle* in both the other two molecular states.† This comes out too when we consider their specific gravities; both these bodies have the lowest specific gravity in the vitreous, and the

* 'Lehrbuch der Anorganischen Chemie,' § 236.

† See Appendix i. Such a *molecular strain* as is here suggested may, it seems, occur and produce its *optical effect* in the mineral, altogether independently of any incipient deformation due to external pressure; an important fact to bear in mind in microscopic petrology.

highest specific gravity in the crystalline state.* The thermal phenomena point the same way. The rapid transition of sulphur from the vitreous to the permanent crystalline state is accompanied with liberation of heat—as has been noticed before; and the heat of combustion of vitreous phosphorus is known to be considerably greater than that of the other modifications of the element, being about one-eighth greater than that of the red amorphous variety.† Both these facts seem to point to such a thing as *latent heat of vitrification*, which will be seen at once to be an important factor in devitrification generally. The work done by it seems to consist in keeping down the molecular architecture of the body to a simple primitive form; more highly complex and more permanent molecules being built up when the latent heat is set free.

We need not confine our attention to the consideration of known metatropic facts in elementary bodies, since a vitreous condition is assumed under certain physical conditions by such compound bodies as arsenious oxide (As_2O_3), metaphosphoric acid (HPO_3), silica ($Si\ O_2$), and borax ($Na_2\ B_4\ O_7 + 10\ H_2\ O$). We must glance briefly at these.

(1.) *Arsenious oxide* in the vitreous or glassy state is well known, and is produced in the refining process by the condensation of the vapour upon the upper heated zones of the iron retort which is used in the process. In time this glassy mass becomes turbid and porcelain-like, as the crystalline texture is developed within it. This assumption of the crystalline form by glassy arsenic may be made to take place so rapidly by the application of a drop of HCl to a saturated solution of glassy arsenic as to develope a beautiful phosphorescent light, through the rapid liberation of the latent heat of vitrification. The quantity of heat set free by one equivalent of arsenic ($As_2\ O_3 = 198$) has been found to amount to as much as 2,652 calories in passing from the vitreous to the crystalline state.‡

(2.) *Vitreous phosphoric acid* (metaphosphate, $H\ PO_3$). This body is best obtained as a transparent solid isotropic mass ("glasige Phosphorsäure") by calcining the ortho-acid in a platinum crucible, and expelling in this way two out of the three equivalents of the water of constitution of the ortho-acid.

	Vit.	Cryst.
* Sulphur,	1·98	2.07
Phosphorus	1·83	2.34

† Wislicenus. *op. cit.*, § 285 (1 gm. of red P. produces 5,592, 1 gm. of vitreous P. about 6,400 calories).

‡ See Wislicenus, *op. cit.* (§ 319.)—"Beim Uebergange der krystallinischen in die amorphe Modification wird nämlich Wärme latent, welche bei der entgegengesetzten Umwandlung in Form von Licht und Wärme wieder frei wird. Diese Wärmemenge beträcht auf die der Formel $As_2\ O_3$ entsprechende Menge (198 Theile) 2,652 Wärmeeinheiten."

Its chief interest for our present purpose lies in the fact that it loses its vitreous condition by absorption of water and changes into the ortho-phosphate, which if dried at 150°C crystallizes slowly in a dry atmosphere as rhombic prisms. Every laboratory-worker is familiar with the fact that sodium-metaphosphate is obtained as a transparent glass when microcosmic salt is calcined. The most interesting fact about the metaphosphates in connection with our present subject is their *polymerism*. Under favourable conditions the metaphosphates are known to assume as many as five different molecular structures, several of which crystallize. This happens:

(a) with dimetaphosphates of the alkali metals;
(b) with trimetaphosphates, particularly those of lead and silver:
(c) with the hexametaphosphate ($Na_6 P_6 O_{18}$) which is formed when fused sodium metaphosphate is allowed to cool slowly. *(Roscoe and Schorlemmer).*

These facts afford a striking confirmation of our previous induction from the behaviour of phosphorus and sulphur, as to the *formation of higher and more complex molecules in the process of devitrification.*

(3) *Silica* ($Si O_2$). Quartz or rock-crystal has a specific gravity 2·6 and is probably in all cases formed out of aqueous solutions* (Wislicenus, *Anor. Ch.*, § 375.) This fact may be compared with the fact that both sulphur and phosphorus crystallize out of solutions in the permanent crystalline form. Glassy silica moreover obtained by melting any of its crystalline forms in the oxyhydrogen flame has a specific gravity of only 2·2, the specific gravities of the intermediate forms come between this and that of quartz (tridymite 2·3, asmanite 2·24). These facts point also to a more stable and consolidated molecular architecture in quartz than in any other modification of the body. In glassy silica therefore we might expect an internal strain or tendency to build up the more stable molecules of quartz. I am not aware if this change has been actually observed.† I have however been fortunate enough to obtain the glassy form of silica in the wet way; that is to say, by leaving the well-known colloid hydrated silica, which was precipitated by just neutralising a solution of sodium silicate ('soluble glass') with HCl, to stand for a long time in contact

* The direct product of combustion of silicon is probably the rare fibrous mineral known as "amianthus" occasionally found in blast furnaces. (See Dr. Percy's Presidential Address to the Iron and Steel Institute, 1886.)

† The late John Arthur Phillips's paper (Q.J.G.S., vol. xxxv.) 'On the History of Mineral veins' is well worthy of study from this point of view.

with water, which gradually evaporated away in the air of the laboratory. When first formed it was clear and transparent with the definite fracture of glass; but in course of time it has lost to a great extent its transparency, and at the edges the fragments have become quite opaque. Prof. Bonney, to whom I gave two or three small fragments of the substance, suggested loss of water as the possible cause, and this turns out to be the case.*

The production of tridymite, apparently by the liberation of SiO_2 from the silicate of alumina contained in fire-clay by ZnO (Zinc-spinell being formed,) has been observed in the walls of the muffels used in reducing ZnO to the metal at Freiberg. *(infra)* The aggregation of silica into crystals in the midst of a mass of kaolin has probably occurred in the china-clay pits of Tintagel, and elsewhere in the Cornish Peninsula. I have some beautiful specimens of these which were given me by the late John Arthur Phillips, the crystals being perfectly bi-pyramidal.

(4) *Borax.* In the ordinary use of borax on the platinum wire the crystalline salt is converted into a bead of glass by the expulsion of its water of crystallization by heat. Its affinity for water however is so great that in moist air it takes up water again and resumes the crystalline form, the mass at first becoming turbid and ultimately falling to pieces.† It can assume two crystalline forms according to temperature:— $Na_2B_4O_7 + 10\ H_2O$ in monoclinic prisms from cold water, and $Na_2B_4O_7 + 5\ H_2O$ in octahedra out of water above 60°C.

(5) Calcium fluoride affords such a marked illustration of metatropic change, that it ought to be added to the cases there cited. It occurs native only in the crystalline form, either cubic or octahedral.

It can be artificially prepared:

(*a.*) As a *gelatinous opalescent* mass it is formed by the action of a solution of K F or Na F upon $CaCl_2$ in solution.

(*b.*) As an *amorphous powder* it can be obtained by the decomposition of $CaCO_3$ by aqueous H F. The fact that this amorphous CaF_2 can be converted into octahedral crystals by heating it to 250°C in a *closed tube* with very dilute H Cl is a noteworthy instance of metatropy.

(*c.*) As *regular 8-hedral crystals* it may be prepared (as well as SrF_2 and BaF_2) by melting the carbonate with Na Cl + Na F.

In (*b*) the tendency of hydrostatic pressure to promote crystallization is strikingly illustrated.

In the case of vitrification of As_2O_3 the temperature requires to be just high enough to prevent crystallization. Experiment has shown that this requires a little over 200°C if the temperature is maintained a fairly long time. Vitreous As_2O_3 can be produced more quickly by heating the substance under the pressure of its own vapour in a sealed tube. At rather lower temperature the rhomboëdral mineral Claudetite is occasionally formed. Bodies which, like As_2O_3 and ammonium salts, sublime under ordinary pressure seem to lend themselves to the investigation of the solid-liquid 'critical state.,

* See Appendix i.

† Devitrification may be seen even macroscopically in a few hours, when a borax bead" is exposed to ordinary atmospheric moisture.

From the facts which we have now reviewed in connection with devitrification we seem warranted in making the following inductions:

(1) *The vitreous state of a body represents a more primitive and simple molecular structure with a corresponding low degree of stability.*

(2) There is such a thing as *latent heat of vitrification,** the loss of this heat being accompanied by the building up of more highly-complex and more stable molecules with a tendency to assume a crystalline form.

(3) In some cases (as exemplified by Si (OH_4), H P O_3 and borax) hydration or dehydration as the case may be appears to be a factor in the process of devitrification.

From a somewhat different line of observations I have been led to see the necessity of recognising two distinct modes of devitrification, as Prof. Bonney has suggested (Address, 1885, p. 67). May we not distinguish these as

α. hydato-devitrification,

β. thermo-devitrification,

and the converse in each case?

Hydato-devitrification again may, it appears, occur in two ways:

(1.) by taking up H_2O, hydro-devitrification:

(2.) by loss of H_2O, dehydro-devitrification.

It is not unlikely that the non-recognition of (α) may have misled observers in some instances, by suggesting changes requiring very high temperatures, where no such conditions have really supervened in the history of the rock in question.

In deep-seated rocks there is really no reason why, below the 'water-line of permanent saturation' (Prestwich, Geology, Vol. i., pp. 164, 336), silica should not remain for a great length of time as a colloid or imperfectly rigid glass;† in fact it is difficult to see how in some cases dehydration could happen at all. This would be favourable to metataxic change without discontinuity of the individual masses of the silica in such rocks.

An extension of the idea of allotropy to mineral chemical compounds and the recognition of their susceptibility to metatropic change, along with the general lowering of their fusion-temperatures as the result of the latent heat contained in them in the vitreous state, is seen to throw considerable light upon some of the obscure phenomena connected with the mechanical deformation of composite rock-masses without crushing.

If we regard a glass as (*per se*) a (mechanically) stable liquid, that is one in which the latent heat of liquidity is so far withdrawn that the molecules have acquired such fixed relative positions that these cannot be altered without fracture, may not movement of individual *atoms* still be possible, so as to build up more stable configurations within the individual molecules, intra-molecular

* I would suggest here (and especially in connection with the case of As_2O_3 cited) that this may throw some light upon the luminosity which calcite is said to exhibit after cooling, below a state of fusion at a glowing heat in contact with metallic iron; also upon the recalescence or after-glow of iron itself in cooling from a bright red heat.

† This idea suggested itself to me in the study of Prof. Bonney's description of the structural peculiarities of the Tor Cross Rock. (Q.J.G.S., vol. xl., p. 15.)

as distinguished from inter-molecular energy still operating? Dr. Percy in the volume 'On Fuel, &c.' (p. 54) of his 'Metallurgy,' states on the authority of Fournet that 'the same silicates are more fusible in the vitreous than in the crystalline state,' also that "when devitrified glass is heated it does not soften before melting, but passes suddenly into the liquid state." He has also observed acetic acid crystals retaining their form permanently at temperatures at which the same acid when liquefied would not again crystallize. Now since we have no reason for supposing that there is any difference in the amount of heat latent in a given mass of a liquefied silicate, whether liquefied by the fusion of the vitreous or crystalline form, it follows that the *excess* of the quantity of heat applied to liquefy a given mass of a given silicate (or mixture of silicates) when crystalline over that required to liquefy the same mass when in the vitreous state must represent the *latent heat of vitrification*. This seems to furnish direct proof of the existence of such latent heat, and to suggest a method for its measurement.

In the light of the theory put forward in my paper on 'Dissociation and Contact-Action' (*Chemical News*,* vol. liv, No. 1402), as well as from considerations already put forward in this note, it seems reasonable to regard this residual latent heat as intra-molecular (atomic) kinetic energy. On the principle of 'conservation of energy' it is represented by the equivalent of work done in keeping down the atoms of the individual molecules in a lower (less stable) state of combination, giving freer play to atomic forces.

A consideration of the known properties of artificial glass throws some additional light upon the subject.

(a) In the dry way. The tension or strain to which unannealed glass owes its brittleness is illustrated in a remarkable degree by the disruption of a 'Rupert's Drop,' when the point is broken off, from the extremely unstable molecular condition in which the surface particles have solidified. The devitrification of glass and its conversion into 'Réaumur's porcelain,' by heating the mass strongly for a considerable time embedded in sand or gypsum, was known a century and a half ago. There have been various speculations (by Dumas and others) as to what the precise nature of this change may be, but they have now little more than a historical interest. Among later workers Benrath has found that glass which contains more silica than is represented by the formula $M_2Si_3O_7$ readily becomes devitrified. Leydolt maintains the existence of obscure crystals† in ordinary glass, which have formed in a super-saturated non-aqueous solution, since a crystalline texture can be detected by the microscope on the surface of all melted unpolished glass after contact with strong hydrofluoric acid, and washing with weaker mineral acids. Péligot states that the melting point of devitrified glass is higher than that of the vitrified portion; a fact which again suggests the idea of latent heat of vitrification.

* *Corrigenda* in that paper:
p. 179, col. 2, line 21 from *bottom*, for 'variations' read 'vibrations.'
p. 180, col. 2, line 29 from *bottom*, for 'solubility' read 'stability.'

† Probably mere crystallites or belonites as in the case of sulphur described above. It is very probable that these forms are developed (as Prof. Bonney has suggested to me) by the action of the strong acid upon the glass, since this is a mixture of silicates.

(b) In the wet way. Ordinary soda-lime or potash-lead-glass is not only attached by acids, but is easily decomposed, when placed for some time in contact with boiling water. Stas has found that glass containing lead or alumina (the latter found in almost all vitrified rocks) is readily acted upon by acids, while Bohemian glass being rich in silica and free from alumina withstands this action best. Here we have two agencies: *(a)* heated water; *(b)* acids capable of decomposing a vitreous mineral. It requires no stretch of the imagination to see how both of these may be present in the interstices of rocks in the earth's crust to aid in the work of devitrification; nor must we forget the enormously increased value of the *factor of time* to be allowed for in lithological changes.

The devitrification at the surface of glass which has been buried for years in the surface-soil would seem to point to the action of the humus-acids (which Julien* has shown to be in the presence of ammonia great natural solvents of silica) as playing a very important part in devitrification at no great depths in the earth.

The action of *superheated water* in causing devitrification—doubtless by the enormous increase of its solvent power—has been fully proved by the splendid experimental work of M. Daubrée. By heating glass in water in powerful tubes and maintaining them at a temperature of 400°C with an internal pressure of over 1,000 atmospheres for several weeks he succeeded in completely decomposing the glass, the decomposition-products consisting of—

(1) alkaline silicates which remained in solution;
(2) an amorphous opaque mass with a slightly fibrous texture but otherwise resembling kaolin;
(3) crystalline quartz, which so closely resembles native quartz in crystal-form, cleavage, optical properties, and even in the inclusion of globules of water, as to be undistinguishable therefrom. (*Etudes experimentales*, pp. 159-166.)

Daubrée further found that "the vitreous volcanic rocks (obsidians and perlites), when submitted to the action of superheated water, appeared to behave in a manner comparable to that of the artificial glasses" (p. 175). Assuming, as we may, in cases of devitrification of the glasses of the older eruptive rocks a considerable enlargement of both the factors of time and temperature (the water possibly in some instances attaining a red heat),† the experiments of M. Daubrée would seem to

* "The Geological Action of the Humus Acids." (A. A. Julien) Proc. Am. Ass. Science, 1879.

† See Pfaff: *Geol. als. ex. Wissenchaft*, pp. 139-141.

throw great light upon devitrification at depths, resulting in the formation of perhaps some felsites and porphyries.

Bonney (Presidential Address, 1885, p. 66) mentions "a fair quantity of earthy dust" along with globulites and belonites in a piece of glass which had been heated for three weeks in a crucible to a bright red heat. This seems to be a parallel case with the production of amorphous sulphur described above (page 37.) Prolonged heating seems in both cases to have produced *intra-molecular* change by bringing the *atoms* into new relationships with one another; whereas in an ordinary case of fusion followed by rapid cooling the changes would appear to be rather of an inter-molecular nature. Time is thus seen to be as important a factor in the thermal dynamics of rocks as in ordinary dynamical phenomena. It is the conversion of the absorbed luminous solar rays into atomic kinetic energy which probably explains the formation of amorphous sulphur on the surface of a molten mass congealing in strong sunlight. The developement of very minute acicular crystals in the sheets of window glass, described on p. 65 of Bonney's Address, was probably due to the action of traces of atmospheric moisture condensed on the surfaces of the plates of glass. This probably had something to do also with the developement of the same structural character in the case of the piece of glass described on p. 66, though it is not easy to suggest how this could have taken place without knowing all the details from the beginning to the end of the experiment. The comparison of those cases with that figured by Daubrée (*Études Experimentales*, p. 171) is interesting.

M. Daubrée says of the amorphous mass ('le residue fixé') mentioned (2) in the reference to his experimental work, that it is found on analysis to be chiefly a mixture of hydrous silicates (p. 158), somewhat porous (p. 159), and that a comparison of the analyses of the ordinary glass and the decomposed glass shows that "le verre a perdu environ moitié de la silice et un tiers d'alcali, et que le nouveau silicate a fixé de l'eau," (p. 162). He also points out the zeolitic character of the hydrated silicate. On p. 179 he makes the following significant remark:

"En voyant le quartz se séparer si facilement du verre, il est impossible de ne pas reporter sa pensée sur les veines de quartz qui sillonnent les quartzites et les phyllades, et qui sont probablement formées, comme dans l'expérience, aux dépens des roches avoisinantes."

How near he came to the recognition of the potent action of water in the 'critical state,' is seen from the following passage (pp. 169, 71):

"Dans les expériences dont il s'agit, les deux tubes n'etant pas complétement remplis d'eau, le tube de verre ne peut plonger dans le liquide que par sa partie inférieure, aussi bien á l'intérieur qu' á l'extérieur. Cependant, il est toujours attaqúe avec uniformité dans toute son étendue. Ce résultat prouve que, dans les conditions où nous avons opéré, la vapeur d'eau, *par suite de la température, et de la densité qu' elle acquiert*, agit chimiquement comme l'eau liquide. On entre alors dans un état de choses où la voie humide vient presque se confondre avec la voie sèche."

Dr. Percy in the first volume of his *Metallurgy** has given some very interesting information about glasses and slags, which ought to be known to every petrologist; and I have seen specimens of devitrified glasses of extraordinary interest and beauty in his splendid collection. Analyses of both the original glass and of the devitrified (crystallized) nodules are

* London, John Murray, (new edition), 1875. See also his remarks (p. 54) on the "Conditions which seem to be essential for devitrification."

quoted by him. These were made independently by Kersten and Terreil. The latter analyst, by comparison of the results of the analysis of the glass with the analysis of the materials from which it was made, found that in the process of crystallization there was no loss by volatilization. Dr. Percy points out (p. 53) that the "crystallized glass may be regarded as augite in which a portion of the magnesia is replaced by soda." A comparison of the two analyses shows that the crystallized portions are richer in lime and magnesia, and poorer in silica and alumina, than the vitreous portions. This may perhaps be accounted for by the unequal mixture of the ingredients in the molten state.

It has been suggested by some writers that glass, being a mixture of silicates, may be regarded as a solution of one silicate in another. If so this may throw some light upon the devitrification of natural glasses. But that it is not a necessary condition of devitrification generally is clearly proved by the facts reviewed in this work in connection with devitrification of some elementary bodies (sulphur, phosphorus) and of silica and arsenic. (See Appendix i.) Zirkel* refers to an ingenious device of Vogelsang for illustrating the idea just referred to. Vogelsang made separate solutions of sulphur and Canada-balsam in carbon bisulphide; he then mixed the two and allowed a drop of the complex solution to concentrate on a glass slide by evaporation of the carbon bisulphide. In this way he obtained rhombohedric crystals of sulphur in a glassy matrix. Greater interest however attaches (in the author's mind) to the cases described in this work of the *crystallization of sulphur out of a matrix of the sulphur-glass itself.*†

In general (as Rammelsberg in his *Mineralchemie* points out, p. 39) a body in the 'amorphous' (vitreous) state is more easily attacked by reagents than in the crystalline state. As examples, he cites garnet, vesuvian, epidote, axinite; all of which furnish on melting *glasses*, which are decomposed by acids with the separation out of gelatinous silica. To these may be added the remarkable fact announced by Crookes and Tidy at the Birmingham Meeting of the British Association (1886), that powdered chalcedony was found to form readily silicate of lead and so to purify water in which salts of that metal were held in solution in minute quantities, while crystalline quartz sand had no such effect.

Again, in such a mixture of crystalline and vitreous ('colloid') silica as is presented to us in common flint (as it occurs in the chalk), something of the same sort of thing may be observed. Long observation of the degraded flints of the

* *Die Micros. Besch. der Mineralien und Gesteine*, p. 95.
† See Appendix i.

gravels of the Bagshot country has made one familiar with a great variety of appearances and mineral characters which flints are capable of assuming, in what is usually called 'weathering.' This includes the long-continued action of peaty waters or of humus-acids contained in the soil, which with ammonia form soluble azo-silico-compounds. In this way the successive layers of which the flint was built up very often around a central mass—a sponge it may be or an echinoderm—are brought out in a very marked manner by the corrosive action of solvents.* In other cases the flints appear to be so completely deprived of their vitreous constituents, that they may fairly be spoken of as quartzite. It is not impossible that some erroneous inferences have been drawn as to the origin of some of the gravels by observers not sufficiently familiar with these peculiarities. All degrees of degradation of flint may be seen in them down to masses which are so devitrified that they may even be mistaken for Sarsen-stones of the more compact variety. On the other hand it would be difficult to deny that the devitrified parts of flint may owe their mineral character in part to direct metatropic change, by the developement of a crystalline structure in the vitreous constituents of the original flint. I have specimens of flint so devitrified on both sides of cracks as to have acquired a stony character through zones several millimetres thick. (See further App. i., *e*.)

Turning again to the facts mentioned in this section of the present work as to the relative densities of the different allotropic forms of the same mineral, we are able to draw from the fact that the *maximum density and stability of molecular structure is identified with the crystalline form the deduction that pressure is favourable to crystallization*. This principle has received experimental verification in the cases of quartz (Daubrée), and perhaps of carbon in Mr. Hannay's experiments in the year 1880 (Note L). And in general it may be said that pressure tends in all cases to make any body pass from a less to a more dense condition, as is well exemplified (*cf*. also laboratory-work recorded in App. i.) in the case of the liquefaction of ice by pressure—a point which I have more fully discussed in its nature and consequences in my paper on the *Mechanics of Glaciers*.† If however we consider for a moment the *kind of pressure* needed it may save us from some false inferences. The pressure which liquefies ice is *hydrostatic pressure*: that is to say, it must act upon the mass

* A fine example of this structure is to be seen in the Museum of Geology, in Jermyn Street; and corroded fragments of such 'banded flint' are by no means uncommon in the gravels of Berks and Surrey. See Roth (*Allgm. u Ch. Geol.* Vol. I. pp. 94-97) on 'Weathering of Quartz and Silica.'

† Quarterly Journal of the Geological Society, February, 1883.

equally in all directions. It must in fact act by way of *compression*, as in the experiments of Sir W. Thomson, Helmholz, Tyndall, and others. The ice must be confined in the cylinder of the hydraulic press, or in some other way. Of course in the early stages of the process we have fracture of the mass, liquefaction at points of contact where the pressure is exerted, and regelation of the liquefied portion as it escapes into the free interstices (as in the well-known copper-wire experiment); and in this way some loss of bulk is experienced. But when the possible limits of this are reached in a closed space, a further exertion of pressure can be made to liquefy the whole mass, provided there is no escape for the liquefied parts.

Just so, in the case of the mineral constituents of a rock composed wholly or in part of vitreous or amorphous material. We have no right to reason from the facts which furnish our data to the inference that mere pressure can promote crystallization, unless that pressure be exerted upon the mass in all directions; in fact, by way of compression. *We have no warrant in assuming that a pressure which crushes a rock will induce crystallization.* It may act as an important antecedent condition by allowing freer access and circulation of water holding mineral salts in solution, and so prepare the way for paramorphic changes; but a pressure so exerted cannot be held to induce metatropic change. When these things are considered, it will be seen that mere deformation of rocks by pressure may have too much attributed to it, and probably has recently had too much attributed to it as a factor in metamorphism. So far as the heat developed by pressure is concerned, this is clearly adverse to crystallization; its direct effect tends in the opposite direction, rather towards fusion than crystallization. On purely physical grounds therefore we may demur to the notion that crystallization is caused directly by deformation of rocks by pressure; although it would be rash to dogmatize on this question until the advance of experimental physics has taught us something definite as to the 'critical state' of the passage of a body *from a solid to a liquid, and the reverse.* Acting in the dry way it would give us a dust, mere mineral matter in a state of molar division: acting in the presence of water, it would certainly help to prepare conditions favourable to paramorphic changes, because it would give us locally conditions (heat, water, pressure) approximating more or less to those which must have prevailed universally at the surface of the globe when the earliest crystalline rocks were formed.

That a mechanically-stable body like glass may not be at the same time perfectly rigid is shown by the well-known fact that mercurial thermometers,

if graduated soon after the tubes are filled with mercury, are liable to give after a time too high a reading (to the extent in some instances of from 1° to 2°), from the compression of the bulb by atmospheric pressure. This brings out very well the importance of the factor of time in vitrification, since it shows that even after a vitreous body has become so mechanically stable as to be highly fragile, a certain change (within very small limits) is still possible, under prolonged strain, for the relative positions of its molecules. Is it not possible that within such limits such forms as belonites may be developed? [A series of observations on thermometers which, having been graduated at once after being filled, some of which were exposed subsequently (others not) to prolonged temperatures at O°C or lower, might give some interesting results as bearing upon the theory of devitrification.]

As regards the necessity insisted upon in this work for *hydrostatic compression* as distinct from mere pressure, in promoting metatropic change quâ crystallization, the idea seems to have been even more strongly put by Prof. Heim of Zürich. From a recent perusal of his great work 'Untersuchungen über den Mechanismus der Gebirgsbildung' I find that that distinguished geologist emphasises, even more strongly than I have done, the necessity for *hydrostatic pressure*. He shows how by this means deeply-seated rocks may have been subjected for lengthened periods to a pressure far beyond the limits of their rigidity (überlastet); and that this has given them a *latent plasticity* which made mechanical (metataxic), and even molecular (metatropic), changes easy in the subsequent massive movements concerned in mountain-building.

The possible 'critical state.' *It is not at all unlikely that *certain minerals* contained in composite crystalline rocks may in some cases have reached this state by the combined action of heat (tending to expand them) with great (hydrostatic) pressure preventing free expansion. This is strongly suggested (*e.g.*) in such flattening-out of masses of quartz as is shown in the Atlas (Tafel vii, figs 5, 6) which accompanies Lehmann's magnificent work *Altkryst. Schiefergest*, as well as by the intrusion by pressure of adjacent mineral particles into them. [The quartz of the larger pebble in fig. 5 shows no such metataxic change, but only signs of fracture under pressure.] It seems almost certain therefore that the flattened pieces of quartz have undergone the particular kind of deformation which they exhibit from their having passed through the 'critical state' (*i.e.* the state in which they were neither solid nor liquid) owing to their having previously existed in a *different allotropic condition* (hyaline or otherwise), which rendered the critical state possible for them, while the temperature was not high enough for the same state to be induced in the neighbouring quartz-masses, in which only crushing effects are seen. A similar explanation may possibly apply to the *deformation by stretching* represented in Tafel viii of the same work. It is not paramorphic but metataxic change which is there observed, for the minerals (*e.g.* the garnets) were evidently there previously completely individualized.† The stretching-out in continuous bands of some of the minerals seems to show metataxis without disruption of molecular continuity, according to the slight variations of the mineral-composition of the bands; and this too may possibly be explained by those in which (under metataxis) the continuity of structure has been most completely preserved, having passed through the critical state. All traces of previous allotropic variation in the quartz-fragments (Tafel vii) may have been disguised by subsequent metatropy;‡ but it is possible to understand how in some

*I had no idea, when this was written, that the subject had been even touched by any experimental investigator. It is therefore very satisfactory to learn that Amagat has shown that carbon dichloride is solidified at a pressure of 900 atmospheres at 10°C, and benzene at 22° C. under 700 atmospheres. He points out that there is "a temperature above which solidification cannot be effected by any pressure; that is to say, a *critical point of solidification*." *Comptes Rendus*, July, 1887, quoted by Sterry Hunt, *Chemical News, October 19th, 1888.*

† I draw particular attention to the crushing, and even pulverization, of the garnets in the non-quartzose layers, and to the preservation of their forms intact in the quartzose layers, as seen (*e.g.*) in the specimen of 'normal Granulit' in the Lehmann Collection in Jermyn Street Museum.

‡ In a note on these, Lehmann says they have 'eine felsitische Beschaffenheit.

cases the presence of traces of CaO or MgO, as in Asmanite, Faserquartz, Chrysoprase (see Rammelsberg, *Mineralchemie*, pp. 163-4) may have increased the tendency to fuse; while the presence of H_2O, as in hyalite (*ibid.* p. 166,) which loses 3-4 per cent on calcining, would seem from my experiments on hydrated glass of SiO_2 (see Appendix i) to facilitate fusion. H. Rose too found hyalite fusible to a porous glass in a porcelain furnace. *

In the light of these three principles we can, I think, explain satisfactorily all the anomalies of structure which have been so carefully worked out by Mr. Teall † (*e.g.*) in the case of the Scourie dyke, as well as those which have been more recently described with great power by the officers of the Geological Survey of Scotland.‡

Thanks to the labours and the keen-sightedness of Allport as an observer, we require no exercise of the imagination whatever to premise the existence, in different parts of the same dyke (even as seen in the same quarry), or other intrusive doleritic masses which have not undergone any considerable deformation by pressure, of such differences in composition and texture (vitreosity principally) as our theory requires. The variable modifications observed in the oldest eruptive rocks (basic and highly siliceous alike) afford no sufficient basis therefore for any theory of 'regional pressure-metamorphism.' The one stubborn fact in the case of the Scourie dyke which cannot be explained away, and therefore presents an insuperable difficulty to the hypothesis that a paramorphic change of augite into hornblende has followed as a *result* of the pressure-deformation of parts of the rock, is the occurrence of the same mineral composition in some undeformed portions of the rock as in those which have been rendered 'schistose.' We may not perhaps find it easy, with ordinary laboratory-appliances, to demonstrate the passage of mineral substances through the 'critical state'; but we have certain and demonstrable data for regarding it as in the highest degree probable. There is no unscientific use of the imagination here: the only data we require are (i) that quartz and most other minerals are fusible; (ii) that their fusing-points are lowered by (*a*) the presence of slight admixtures of fluxing-materials, (*b*) the previous (more or less) vitreous condition; (iii) that when fused at ordinary pressure they expand. These data we have; and the belief in the solid-liquid critical state is as much an inferential truth of science, as our belief (by way of inference from the results previously obtained by Faraday, Andrews, and others) in the

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* Facts of a similiar nature to those just cited from Lehmann were described and interpreted with a far-reaching prescience twenty years ago in connection with the Huronian Series of Lake Superior, by Thomas Macfarlane, Esq., now chief Analyst to the Dominion Government at Ottawa. Under the name of *Slate Conglomerate* (the 'Slate' being a sheared 'greenstone') he has described a schistose rock, in which, wherever the shearing has produced marked 'schistosity' the included granitic fragments are not only rounded off into boulders by the rubbing down of their angles while the igneous matrix was in motion, but in many cases have undergone such an amount of softening that they have been drawn out into pebble-like masses, and even into flat and twisted bands, a parallelism between these and the planes of schistosity being always maintained. In some cases bands of what that author terms 'siliceous slate' are formed by the incorporation by fusion of the siliceous fragments and [subsequent] shearing action; but where the deformation of the fragments without fracture has taken place, I believe that the hypothesis of the solid-liquid critical state furnishes the true explanation. See Mr. Macfarlane's paper on the 'Geological Formations of Lake Superior' in the *Canadian Naturalist* for May, 1867. I have been astonished, on searching in vain for his name in the 'Index of Authors' in either Green's, Geikie's, or Prestwich's Geology.

† Q.J.G.S. vol. xli. pp. 133 *et seq.*
‡ *Ibid*, vol. xliv. pp. 391—395.

liquefiability of the six reputed 'incoërcible' gases, was an inferential truth of science, prior to the publication of the magnificent work of Pictet and Cailletet in December, 1877.

By the recognition of these important physical principles:

(i) the latent heat of vitrification;
(ii) the action of traces of fluxing-materials;
(iii) the lowering of the temperature required for softening by incipient fusion (following as a consequence of (i) and (ii));

new light is thrown over many obscure phenomena exhibited by crystalline rock-masses, which have evidently suffered some deformation through dynamical agencies.

Baltzer's observation that in mountain-movements limestones under great pressure appear to have been the more flexible without fracture the richer they are in argillaceous material (see Kalkowsky, *Lithologie*, p. 284), coupled with the fact that in contact-metamorphism a calcareo-argillaceous cement of a sandstone is often found melted into a glass (*infra*, § vi.), seems to suggest the passage through the critical state (under pressure) of the calcareo-argillaceous material of such rocks, as the possible explanation of the facts observed by Baltzer. This view is further borne out by some facts given by Dr. Percy in his volume on Fuel, &c. (p. 75). Take for example the statement that "any clay whatsoever may by the addition of from half to three-quarters of its own weight of carbonate of lime, be *rendered sufficiently fusible to allow shots of metal to sink through the mass* and collect into a button at the bottom." This state of things would more than meet the requirements of the problem presented by Baltzer's facts; especially when we take into account the fluxing-action (Percy, *loc. cit.*) of 'small proportions of different bases (*e.g.* MgO) which are so often present in limestones.'

The idea of the possible passage through the critical state of certain of the constituent minerals of the rocks is worthy then, I venture to think, of serious consideration, in cases where great mechanical deformation (metataxis) is to be observed, as in crumpling and puckering produced in the great earth-movements concerned in mountain-building, but where at the same time the microscope reveals no traces of molecular discontinuity (crushing) in the mineral bands. In a heterogeneous rock it seems almost certain that this action would be specialized, the induced conditions in a given proportion (heat, water, pressure) causing some of the minerals to pass through this stage and not others. Nor does the fact that the same minerals now exhibit a crystalline texture of necessity militate against this view; since, with a subsequent lowering of temperature owing to the slow dissipation of thermal energy after the pressure-movement had ceased, we should expect the minerals to crystallize. A series of chemical analyses of such portions of deformed rocks as are here referred to (*e.g.* for traces of water of hydration or of CaO or other bases as fluxes in the quartzose folia thus deformed) might lead to some highly interesting results.* It is very likely that the 'mosaic-like' arrangement of the quartz-particles frequently observed is only an incipient stage of contraction after the 'critical state' causing shrinkage-cracks on a minute scale. It is almost certain that in the first stage of devitrification such shrinkage-cracks do occur in some instances (cf. Allport, Q.J.G.S. vol xxxiii, Pl. xx, figs. 1-6).

Assuming that vitrification in the dry way is due to rapid cooling, a little reflection will show us that this admits of degrees. If we take t_1 = the initial temperature of a fused mass when cooling commences, and t_2 = the temperature reached on cooling, we see that for a mass of the same mineral composition and under the same pressure the value of t_1 must be pretty constant in every instance, while the value of t_2 will vary considerably in different instances, and will be determined (according to the general laws of dissipation of thermal energy) by the resulting temperature of the adjacent cool bodies into which the heat of the molten mass continues to pass until equalization of temperature

* Leydolt's researches on Agates (quoted by Zirkel, *Micr. Besch. der Min. u. Gest*, p. 37) seem to give support to this view.

is reached. Again, the rapidity of cooling will depend upon the resultant of the three factors; (1) the mass-ratio of the hot and cold bodies; (2) the co-efficient of conductivity and capacity for heat (the two together giving as the 'diffusivity') of the cold body; (3) the difference of temperature for the time being. In cases where from any or all of these causes the cooling was not so rapid as in others (as in the interior of such considerable masses as have given us the pitchstones and obsidians), the early stages of cooling may have been slow enough to admit of some incipient concretionary arrangement of some of the constituents of the molten mass, sufficient to enable these constituents to acquire such a degree of differentiation as would cause them (with suitable fusion-temperature relatively to that of the residual magma) to undergo incipient crystallization as the temperature fell. Such conditions would probably give us the 'spherulitic structure.' On the other hand, in cases where the value of $t_1 - t_2$ was comparatively large and the cooling very rapid (conditions of more complete chemical saturation of the base by the acids from the accident of their existing more nearly in equivalent proportions in the mass being also unfavourable to segregation) we should have a clear glass, which on subsequent further cooling between the temperature of vitrification and t_2 (in such cases presumably considerably lower), would by shrinkage, followed by the action of aqueous solutions on the shrinkage-cracks, give us the 'perlitic structure.'

A beautiful example of incipient devitrification of the glass, of which the vessels in use in a chemical laboratory are commonly made, has recently come under my observation. An ordinary foot-jar had been used for the preparation of hydrogen persulphide in the usual way, by the action of strong HCl on calcium pentasulphide, the jar itself being placed in a cold water-bath. It was left standing after the formation of the hydrogen persulphide for some weeks, during which all the persulphide formed underwent decomposition into free sulphur and hydrogen sulphide. The jar happened to be slightly tilted in the water-bath, and so many of the minute oily particles of the hydrogen persulphide appear to have lodged on the inner surface of the lower side of the jar. The glass was evidently attacked by these or by the nascent sulphur furnished by their decomposition (or possibly by the Ca set free from the sulphide of calcium); the result being a fine net-work of cracks on the inner surface (often with beautiful iridescent fracture) and quite different from the scratches made near the bottom of the glass by the harder glass of the stirring-rod used. From various points in the walls of many of these cracks fine growths of belonites have set up, which can be very clearly discerned with a moderate power of the microscope, in some instances grouped into little nests. We are furnished here, it appears, with a clear case of *atomic movement in a (mechanically) rigid glass* giving rise locally to change of molecular structure, favoured perhaps by local relief from the general surface-tension of the glass, under the influence of which they had been previously kept in positions of equilibrium.

The same sort of atomic movement is illustrated in the formation of microliths in the midst of clear glass of tachylite. I was fortunate enough to procure a very fine specimen of this mineral some years ago from the basalt of the Falkenlei. The microliths developed in this glass occur isolated and in bundles and bunches, the section furnishing a most beautiful object in polarized light, especially when the microliths are seen in the dark groundmass of the glass between crossed nicols.

I have a specimen of quartz cut for the microscope which, from its illustrating more than one kind of devitrification, seems of sufficient interest to merit description here. As seen between crossed nicols:

(1.) On one side of (apparently) a shrinkage-crack is chalcedony, which assumes in immediate contiguity with the crack an exceedingly fine mosaic-like structure, this structure being more pronounced on the face of the crack, becoming finer and finer as it gradually shades off into the non-differentiated glass of the chalcedony.

(2.) On the other side of the same crack the same mosaic-like structure is seen, but this becomes coarser as we recede from the crack, and presents a gradual transition through quite large plates of clear glass (with very irregular outlines, the lines of fracture being often beautifully iridescent) into a continuous plate of clear transparent glass containing numerous minute belonites.

(3.) Continuous with (2) is a ribbon-like banded structure of agate-chalcedony, in which the bands are for the most part devitrified by the developement in them transversely of an 'acicular mineral,' which reminds one of the case described by Bonney (Pres. Address, 1885, p. 66). This structure, through its whole periphery, shades off into the mosaic-like structure.

The specimen appears to illustrate at the same time two distinct modes of devitrification:

(i.) An anhydrous glass of transparent quartz losing latent (atomic) heat, contracting and assuming the mosaic-like structure (thermo-devitrification).

(ii.) A hydrous glass of translucent quartz developing a fibrous (crystalloid) structure by loss of water (dehydro-devitrification).

When we recollect that the feebly translucent form of silica, chalcedony, is said by Rammelsberg (*Mineralchemie*, p. 163) to lose in some cases as much as 2 per cent. [of Water] on calcining, the hypothesis that the developement of the transverse pseudo-fibrous structure seen in the agate-chalcedony is a case of dehydro-devitrification certainly seems strengthened.

In a paper 'On Underground Temperature' read by Prof. Prestwich before the Royal Society in 1885, some important facts are cited as to the circulation of underground waters, going to show that in massive rocks such as granite very little water passes; while it passes more freely through phyllolithic rocks such as slate, and still more 'rapidly through cross-veins' and through zones of fracture and dislocation. The latter is well known. In this *freer access of water along the junction-planes* between massive crystalline rocks and rocks of sedimentary origin composed of clastic materials we come to see a very potent factor of local metamorphism, secondary paramorphism through the agency of water (especially if facilitated by previous crushing) followed by a welding together (with a certain amount of shearing) of the new minerals, producing zones marked by a certain degree of 'schistosity' (with a rather free use of the term), without producing a really crystalline schist. Some years ago in examining a new section in the railway-cutting at Shap Summit I traced the gradual change by mere 'weathering' from the central exposed mass of unaltered dolerite through partially disintegrated rock into wacke and mere clay; and there seems no reason to doubt that, if this region were subjected to a sufficiently powerful lateral pressure, we should have in this case rocks, that would in some quarters be called schists, formed in the inner zone and a phyllolithic rock (slate) in the outer zone, if such antecedent changes occurred at sufficient depth for earth-movements to operate upon the zone of rock-material. (See further Section vi.) Again, every

field-observer is familiar with the fact that along junction-planes the rocks are generally very much decomposed, and so rotten that (except in the rare cases of fresh and artificially-prepared sections, such as the one just described) it is not often that the true junction is seen ('the place being overgrown') on account of the earthy material formed by the disintegration of the rock on both sides of it; and it is—to say the least—extremely unsafe to *infer a transition* from the clastic into the crystalline rocks from the appearances on either side of, and at some distance from, the true junction-plane. It may not perhaps be too much to say that this is a by-no-means uncommon fallacy running through much that has been written in support of 'regional metamorphism.' And then the further inference is boldly drawn, that the archæan schists, &c., may be only instances on a grander scale of such transitional developement.

There does not appear to be always sufficient recognition of the simple axiom, that where we see combined results it by no means follows that they are referrible to *one cause*, or have even been brought about by the *simultaneous action* of several causes. In the class of instances here considered we see that the combined results of paramorphic and metataxic change may have been produced by several different causes acting in succession, and very often at long intervals of time; the causes which led to paramorphic change having been in operation long before the pressure which has induced the metataxic change (whether cleavage or foliation) was exerted upon the rock-masses affected. This is borne out too by Dr. Sorby's observations upon the microscopic structure of clay-slate, which led him to the conclusion that the individual minerals formed as separation-products in some clay-slates were anterior to the developement of the cleavage, and had only partaken under the influence of pressure of that re-arrangement which we should call metataxic. Zirkel also came to the same conclusion (see § iv.). At the same time it cannot be denied—from the considerations adduced in the foregoing part of this section—that though the paramorphic changes by which the new minerals were actually formed were antecedent to the production of cleavage by pressure, this agency may—in cases where it could act hydrostatically—have helped to produce in them such a metatropic change as would consist of the conversion (or partial conversion) of them from the vitreous or amorphous into the crystalline state.

The physical law that pressure generates heat (as a concomitant of molecular movement) is not overlooked; and it might be urged that this heat might be sufficient to promote formation of minerals of more complex structure

out of those of a simpler molecular structure, as in the formation in the dry way of silicate of lime and other minerals referred to in App. ii, note A; or perhaps in the formation of enstatite (the purest examples of which are found in meteorites, according to Rammelsberg) in the eruptive rocks. *We must distinguish clearly between quantity and intensity of heat (temperature).* In great earth-movements resulting from lateral pressure working slowly and gradually through a great period of time, it may be questioned whether—whatever the quantity of heat developed may be—it would be sufficiently concentrated to give the temperature required by the hypothesis. Exceptional local cases of course may occur where the movement is sudden along thrust-planes, but we must not construct a theory out of these in favour of 'regional metamorphism.'*

If moreover we consider a few of the best-known instances of paramorphic change, one or two considerations are seen to militate seriously against the view. As examples we may take the conversion of augite into chlorite or hornblende, and the conversion of olivine, augite, hornblende, and some garnets into serpentine; also the formation of many micas as alteration-products of older silicates (Rammelsberg). Of the formation of mica in this way, even macroscopically, I believe I have myself observed instances in the slaggy ropy lava-flows of the old volcano Falkenlei near Bertrich, and among the disintegrated *débris* of augitic lavas at Daun.

In all the cases here cited of paramorphic change *water of constitution* is taken up as one part of the process; and ordinary laboratory-experience is opposed to the notion that this is aided by heat. But it might be urged that these changes are presumed to take place under conditions as to pressure of which such experience takes no account. *Primâ facie* this seems feasible; but to this we may fairly object that if such were the case the resultant minerals ought to have a greater specific gravity than the originals, whereas the facts of the case show the contrary. A consideration of the following simple table will put this in a clearer light, the percentage of water of constitution being given as the mean of a number of analyses, in the case of each mineral, as given by Rammelsberg *(Mineralchemie)*:

No more can we by any process of sound inductive reasoning build up such a theory upon any foundation in the facts observed in such folded anticlinals as occur *(e.g.)* in the Alps, in the Urseren Thal between the St. Gotthard and Finsteraarmassif, and between the St. Gotthard and Tessinamassifs, on either side of the Gotthard-axis of elevation. On a 'regional' scale the real problem to be solved is the genesis of those huge massifs themselves, and of the characters they possess in common with the great archæan crystallines in all the four quarters of the globe, and not the origin of such inconsiderable localized masses of altered sedimentary materials as have been here and there squeezed as in a vice between them. (See further App. ii, Note P.)

Mineral.	Water of Constitution	Specific Gravity.
Garnet	0 per cent.	3·2—4·3
Olivine[1]	0 „	3·4
Augite[2]	0 „	3—3·5
Hornblende[3]	generally less than 1 „ rarely, 2 „	3
Muscovite	4·08 „	2·9
Chlorite	12·11 „	2·8
Serpentine	13·08 „	2.6

[1] *Chondrodite* (of close mineral affinities with olivine) contains no water. *Villarsite*, which agrees with olivine in crystal form and optical characters, contains 4 to 6·2 per cent. of water, with a corresponding diminution of density to 3·04.

[2] Occasionally a trace of water, rarely as much as 1 per cent.

[3] Weathered varieties are found to contain 3 to 20 per cent.

The obvious general conclusion on this point would seem to be, that though pressure acting hydrostatically is favourable to crystallization it can only be safely asserted to promote this by way of a *metatropic alteration* of bodies whose chemical composition has been previously determined by various paramorphic agencies; that its action in this respect is limited to those cases in which crystallization is accompanied by increase of density of the body which crystallizes; and that this stops a long way short of what is required of it to bring about those vast and complicated changes which are implied in the 'metamorphism' of clastic sedimentary rocks into crystalline schists and gneiss.

§ iv. METATAXIS.

This term has been already defined as connoting alterations in the relative positions of the constituents of a rock-mass; and the developement of slaty cleavage, which is now pretty generally recognized as a result of mechanical force, has been instanced as typical of metataxic change.

a. Cleavage.—The recent exhaustive paper on this subject by Mr. Harker* has put the matter in a very clear light. While agreeing with that author in his general treatment of the subject, and in his general conclusions (so far as cleavage-proper is concerned), I submit the following remarks:—

* British Association Report (1885), Aberdeen Meeting.

Instead of contending for the absolute truth of one particular form of the ellipsoid of strain, it will be seen that in some instances (from variations in the determining factors) Dr. Haughton's view might be true to the facts of nature, as represented by an ellipsoid of rotation; *i.e.* with equal axes parallel to the 'end' and 'side'; while it would fail to be true in cases where the movement of the constituent particles was free only along the line of dip. Resistance being in the latter case offered by more rigid masses of the crust of the earth on either side of the region or zone of compression, the movement in the direction of the line of strike of the cleavage might be sufficiently hindered to allow of no bulging-out in that direction. This would (the operating force being presumably the same in both cases) give a greater elongation to the ellipsoid in the direction of the cleavage-dip. On the other hand, if the particles were comparatively free to move in the direction of the cleavage-strike, the portion of the compressing force expended in moving them in that direction would of course not be available for moving them in the direction of the cleavage-dip. In this case, the striation or 'grain' of the slate would be less pronounced.

Again, the ratio which the extension of the rock-mass along its line of cleavage-dip would bear to that of compression on the one hand, and of extension in the direction of the cleavage-strike on the other, must be in part determined by the relation which the pressure might bear to the dead weight of the superincumbent mass. It is conceivable that at great depths lateral pressure might result in producing a cleavage-structure in a plastic or pasty rock-mass, under such a dead weight of the superincumbent mass that the movement of the constituent particles of the rock in the direction of the cleavage-dip would be so far retarded that we should not have a strain-ellipsoid at all, but an oblate spheroid of stress. Mathematical ways of looking at natural phenomena give a certain fixedness and precision to our ideas; but nature is not bound by them; and there is certainly a danger of the mind which is too much entrammelled by them losing some of that elasticity which it might otherwise apply to the study of natural phenomena.

In dealing with purely geometrical considerations,—as (*e.g.*) in determining the centre of gravity of a solid homogeneous body—the mathematical conception of the body as made up of indefinitely thin parallel planes is no doubt a safe one; but in considering the causes which have induced such metataxic disposition of the constituents of a rock-mass as are exhibited in slaty cleavage, we are dealing with dynamical phenomena on a minute scale. The question arises, whether our conception

of such shearing-planes as are postulated in Mr. Fisher's theory, which attributes cleavage to the shearing-action arising from the settling down of the more central parts of an elevated mass—a sort of magnified 'cone-in-cone' structure—is anything more than a mental fiction. A negative answer to this question would seem to be furnished by the simple consideration that such a strain as would be caused would be *continuous* through the mass, and would result rather in a slight (perhaps hardly perceptible) strain-structure, rather than in that discontinuous mode of metataxis which we recognize in slaty cleavage. And there is the further fact militating against Mr. Fisher's theory, that it is generally (though not universally) found that it is in the oldest argillaceous rocks, such as the Cambrian and Silurian, that the phenomenon of slaty cleavage is most distinct and perfect. It seems to be sometimes forgotten that such rocks are now found near the surface of the earth as the result of extensive denudation; they are but parts of the worn-down stumps of ancient mountain-regions from which some thousands of feet of superincumbent strata have been removed since the cleavage was induced upon them. But if Mr. Fisher's theory were the true explanation of the fact, we should expect surely to find things exactly the reverse of this.

It has been pointed out above that the settling-down of the more central parts of a mountain chain would tend to produce rather a feeble strain-structure than cleavage. This implies of course that the materials were still in such a plastic state as to admit of such a metataxic change occurring. In other cases the rock, from loss of interstitial water after its elevation (as pointed out long ago by Von Cotta), might become too brittle to allow such an alteration in the relative position of its particles, or even bending and contortion; and then we should have an indefinite number of small fractures combined with a certain amount of sliding and possibly slickensiding along some of the cleavage-planes, which would give us (with subsequent cementation) the phenomenon of a number of minute step-faults.* This would furnish literally a case of 'Ausweichung,' and so the term '*Ausweichungsclivage*' would be fit and appropriate. It is unlikely that any great mass of rock would become equally rigid throughout at the same time; and so we should be prepared to find that in the settling-down of the central parts (a fact it would be difficult to deny) the

* The mode of fracture and recementation here referred to is a matter of common observation. I have seen it exhibited in the Cumberland slates and even more extensively in the limestones of the Northern Alps, both in the rock-mass and in boulders. The recementation usually occurs in the limestones through the deposition of calcite. It is a different thing from a recemented 'crush-breccia.'

parts which remained more plastic would undergo minute contortions, such as those figured by Mr. Harker in his invaluable memoir (Figs. 11 and 12); while the gritty bands in the slate, possessing less cohesion, would furnish zones of least resistance to the lateral strain, the angle of their previously induced cleavage being changed without that cleavage being of necessity obliterated. This would give us the "zigzag arrangement" noticed by the same writer (Fig. 13). While then there would seem to be insuperable difficulties in the way of attributing cleavage-proper to the cause assigned to it by Mr. Fisher, it would seem that there are other and subsidiary phenomena which may be attributed to its operation; and if we can see in this way the work done by its operation there remains the less necessity for seeing that work done in producing the primary cleavage itself. In the present state of our knowledge of this subject it seems to be pretty generally acknowledged that *pressure* is a sufficient *vera causa* for the phenomenon of cleavage;* the fact that the cleavage has been induced subsequently to the displacements of the rock-masses by contortion and faulting simply implies the continued operation of lateral pressure after those limits were reached, within which displacement of the rock-masses in each case was possible. No general rule can be laid down as to what those limits would be. If the pressure ceased before those limits were reached we should have (as is so common in the Secondary Rocks) ordinary contortion or faulting (or both), without cleavage; but in such a case we clearly get no cleavage because the *force in operation is being expended in doing the work of displacement*, and consequently is not available for the other kind of work at the same time.†

To put it another way,—while the work of mere displacement is going on, the action of the lateral thrust is greater than the reaction due to the resistance of the mass; but when that mass has by crumpling and fracture (both on the faulting and on the crushing scale) been so rammed together that further displacement of rock-masses is impossible, the reaction equals the action of the thrust, and then cleavage-structure begins to be set up. The production of cleavage after the displacement of the rocks *en masse*, which the

* See App. ii, Note M, on the distortion of fossils.

† It will be recollected that the same line of reasoning was adopted by the writer to explain the distribution of, and the work done by, the mechanical force of the moving mass, in the 'Mechanics of Glaciers' (Q.J.G.S. February, 1883). It is satisfactory to note that the main contentions of that paper are borne out by the work of Prof. Penck, in the Northern Alps, and by the thorough-going work of Prof. J. W. Spencer, in Norway, (see his 'Glacial Erosion in Norway'; *Proc. Roy. Soc.*, Canada, 1887). The latter author has however slightly mis-quoted me on the matter of solar radiation.

generally-observed relation of the cleavage-planes to the contortions plainly indicates, is thus found susceptible of explanation, and lends no support to Mr. Fisher's theory as to the origin of the cleavage.

It is with some diffidence that I venture to say that Prof. Heim's arguments (*Mech. der Gebirgsbildung*, Bd. ii, p. 12) by which he supports the proposition, "Alle Umformung . . . . . es heute vor uns stehen," seem to me not altogether convincing. Still less convincing do they become when one works through the details of his illustrative figures (ATLAS, Tafeln xiv, xv) in the full recognition of the vast *physical* difference there is in the molecular structure of the same chemical body according as it is crystalline or amorphous.* The difference in the molecular mobility (especially with access of water) in the two cases is enormous. The expression "in gleichem Grade fest und hart" requires this large qualification as well as that implied in the qualifying word "annähernd."

The flattening-out of the particles and their general re-arrangement with their longer axes in the direction of the cleavage-dip can scarcely be said to receive much elucidation from Tyndall's illustration of the action of the rolling-pin in producing the laminated structure of puff-paste, since we are not at liberty to postulate the operation of such a force as 'rolling-friction' in producing slaty cleavage.

In Ramsay's Memoir on 'The Geology of North Wales' some interesting instances are described of cleavage induced by pressure upon the volcanic ash-beds of Snowdon. In these cases no mention is made of the occurrence of accessory minerals on the cleavage-planes, nor of anything approaching to foliation. In the case of the 'porphyries' which are stated to be thus affected by cleavage in common with the altered ash-beds there is some obscurity arising from the dubious nature of many rocks which in the older nomenclature have been described as porphyries; but in the case of the cleavage of the ash-beds there would seem to be little difficulty in understanding its production, when we recollect the great permeability of volcanic ashes to water. This has been recently pointed out and important deductions drawn from it as the main factor in determining the intermittent action of volcanoes, by Prof. Prestwich in a paper read before the Royal Society in 1885. Given sufficient time, pressure, and temperature (as determined by depth) we have all the factors needed for the paramorphic changes called into play in converting a loose incoherent mass of volcanic ash into a hard compact semi-crystalline rock, which gives these Snowdon altered ashes great power of resistance to the hammer (as I

* My impression is that the figures certainly indicate a bulging inwards of the slaty matrix of the rock between the displaced segments of the Belemnites indicating plasticity, and this impression has been confirmed by an examination of the specimens which Dr. Heim exhibited at the International Geological Congress in London, 1888.

know from personal experience); while the ash itself retains distinct traces of its original stratification. Among the paramorphic changes wrought by the action of heated water would be the separation-out of silica, to form the cementing material to which the rock owes its present indurated character. If, while this process was going on, the mass was subjected to great lateral pressure, the silica being still to a great extent in the colloid state, it is easy to see how the cleavage structure may have been caused; and there remains no necessity for inventing a theory to account for the production of cleavage in a rock of the indurated character which these ashes now possess. It is possible that the pressure itself which produced the cleavage may have helped the process of induration by the expressure of the water of hydration of the colloid silica, thus effecting a certain amount of metatropic change as well as a metataxic change in the rock mass. It would thus seem that cleavage instead of crushing has resulted, because the rock was not at the time sufficiently rigid for the latter result to occur. In this case we look in vain for any paramorphic change which we can attribute directly to the action of pressure.

There are two other considerations which militate strongly against the view that the accessory crystalline minerals found on the cleavage-planes of many slates are the result of paramorphic changes induced by the pressure itself.

(1) These minerals have generally a definite orientation in the direction of the cleavage dip, as if in the shearing process which produced the cleavage they had moved into the position of least resistance before the rock-mass became too rigid to allow such movement.

(2) Similar minerals are found (as before noted) as separation-products in laminated shales (Schieferthon) and on the cleavage-planes of true slates (Thonschiefer). The following are noted from the last edition of Credner's Geology (p. 123):

Accessory Minerals in Thonschiefer. *(Silurian and Devonian.)*	Accessory Minerals in Schieferthon. *(Secondary and Tertiary.)*
Rutile (as prismatic needles parallel to the cleavage).	Microliths of hornblende.
Plates of green and yellowish mica.	Scales of potash-mica.
Scales of felspar.	Particles of quartz.
Oval and roundish grains of quartz with numerous fluid inclusions.	Minute laminæ of iron-glance.
Pyrites.	Brownish and greenish needles of undetermined nature.
	Pyrites.

Von Hauer *(Die Geologie,* p. 55) mentions the common occurrence of such minerals both in Thonschiefer and Schieferthon.

Zirkel *(Die Microscopische Beschaffenheit der Mineralien und Gesteine,* Leipsig, 1873) describes the occurrence of such micro-crystalline constituents of Thonschiefer. Of the alternative views as to whether these minerals previously existed in the unaltered rock, ('vor seiner Verfestigung'), or were formed on the other hand subsequently by metamorphic processes ('metamorphische Vorgänge'), he decidedly inclines to the former view. His words are (p. 494): "Jede sorgfältige Untersuchung der Beschaffenheit der Dünnschliffe, jede vorurtheilslose Betrachtung der Anzahl, Lagerungsweise, und Vertheilung der krystallinischen Elemente, welche sich schwerlich erst in dem starren Gesteine hinterher entwickelt haben, hat bis jetzt mit der Ueberzeugung geendet, *dass der erste Theil jener Alternative ebenso warscheinlich, als der letztere unwarscheinlich ist.*"

A distinction between the phyllites and Thonschiefer (slates) is thus drawn by Kalkowsky *(Lithologie p. 256)*: "While the phyllites (Urthonschiefer) are throughout purely crystalline rocks, the Thonschiefer consist mostly of derived (allothigenous) constituents as well as of authigenous, though there are cases in which the former appear to be wanting." He also states that "a widely-spread constituent of Thonschiefer is a colourless or faintly-coloured *micaceous* mineral, the feebly-refractive scales of which usually lie parallel to the cleavage-planes. Such scales are probably in many cases derivative (allothigenous); the chief part of the micaceous mineral appears however to be in many cases authigenous."

Bonney (Pres. Address, 1886, p. 66) says that his observations fully "confirm Sorby's statement concerning the presence in many cases of exceedingly minute flakes of a micaceous mineral in large quantities;" and hints at their possible derivation from the felspathic materials in the original clay. "Still exogenous (allothigenous) mica may be often observed." He remarks that "the crystallization of the mica is no doubt a consequence of pressure" (metatropic change). The mineral as a chemical body was evidently there (in the case of secondary or authigenous mica resulting from secondary paramorphic change) before the cleavage of the rock-mass was induced by pressure.

β. Crumpling and gnarling. Another variety of metataxis is seen in the crumpled and gnarled condition of the folia of many gneisses and schists. That the pressure which caused this has not been the cause of those paramorphic changes by which the minerals of the rock were formed is seen in the simple fact that its action was (at least in a very large number of instances) posterior to their formation. It must inevitably occur that in the history of such rocks they have been locally subjected to a *ramming* process, the pressure acting in one direction upon them for a length of time, while the reaction due to the resistance of bounding rigid masses was exerted upon them in several directions. We may illustrate this by the compression which a lump of clay suffers when it is forced into a brickmaker's mould. If the clay is homogeneous we get a homogeneous brick; but with a

laminated clay which has been imperfectly ground and mixed the case is different. Of this I have lately observed some instances, in the case of bricks which have been undergoing weathering in the face of a wall for some twenty-five years in this neighbourhood, and were made—as I have ascertained—from the laminated clays of the Bagshot formation. The surface of these bricks exhibits a contorted and gnarled structure as complete and marked as is to be seen in any schist or gneiss that I have ever observed. The heat of the kiln has in this case rendered the contortions due to compression permanent; but it has imparted to the mass nothing of the nature of foliation.

Interesting examples of crumpling, with parting-asunder of the folia at the acuter portions of the curves and filling-in of the interspaces with injected granite (Granit injicirt) are given by Lehmann (Atlas, Taf. ii, fig. 1; Taf. xiii, fig. 2). On Taf. xvi, fig. 2, is shown a very interesting example of "Granulit durch Stauchung zerissen, die Lücken *mit secundären Quartz erfüllt.*" A comparison of such beautiful examples of crumpling without fracture (bruchlose Faltung) as are given on Tafel xvi, fig. 4, Tafel xiii, figs. 1, 2, 3, Tafel xix, figs. 2,5, with the examples of crumpling with fracture given on Tafel xvi, fig. 1, Tafel xiv, figs. 1, 2, is very instructive. A slight difference in the previous allotropic condition of the minerals (chiefly the quartz) and the consequent difference in the degrees to which they lent themselves readily to the critical state, seems to have made all the difference in the two phases of change illustrated in these two series.

The frequent flexure (and contortion) of massive quartz veins in the phyllites and slates of the Alps, such as one often sees in the fine artificial sections made in road-cuttings, without any apparent signs of fracture, really presents no difficulty, when we allow for such slight metatropic change in the quartz itself as might easily result from the loss of water during the later process of induration resulting from desiccation, which they have undergone in common with the mountain-masses in which they occur, since their upheaval. Laboratory-experience (see App. i) bears out this view.

γ. *Foliation.* Teall* has lately described an interesting experimental illustration of the effect of pressure upon a mixture of clays differently coloured, in inducing 'parallel banding' in such a mass. But a little reflection shows that this does not carry us very far as a clue to the way in which the foliated structure of the schists and gneisses was brought about. For, (1) some distinction (if any) should be noted between the effects of squeezing and rolling; (2) we can hardly (as before pointed out) invoke the action of rolling-friction on any large scale to produce the shearing which is probably the immediate cause of the lamination or parallel banding observed; (3) the application of pressure by rolling is clearly intermittent, and so time is allowed between successive applications of pressure in this fashion for the

* *Geological Magazine*, December, 1887.

movement of interstitial water and the action of capillary forces to bring about a transfer of the colouring-matter; (4) the real *crux* of the problem—the conditions which caused the assumed antecedent pasty or yielding condition of the rock-mass—is left unexplained, and we must look for it in another direction. Let us consider the fact that recent extensions of our knowledge of the history of the Earth's crust tend to show that the movements which have resulted in the formation of the central cores and the great mountain back-bones of our present continents, and of continental tracts of which only fragmentary portions now remain as 'dry land,' were in most cases initiated at an early stage, probably before the close of the Palæozoic Period, to take a wide limit. This not only follows as an inference from the vast amount of accumulated material which has been derived from them to form those subsequent sedimentary deposits to which we cannot assign an organic or chemical, but only a mechanical origin; we have direct evidence of it in the vast amount of rolled detritus found all over the world among the palæozoic formations. As a few examples of this we might point to the great breccias and conglomerates which mark the latest palæozoic epoch (the Post-carboniferous) in the Rothliegendes of central Germany, in the great zone of such derived and rolled materials which marks the same epoch in the Alps from end to end of the system; in the great 'Permian' conglomerates of the West of England and (according to the view put forward by me elsewhere) of the Devon area.* Everywhere the derivative relation of the materials of these detrital rocks to the palæozoic and archæan rocks of the adjoining regions is the same. And, as is well known, conglomerates and breccias occur more or less in all the palæozoic formations down even to the Cambrian, which are quite distinguishable from the volcanic breccias of those same formations, with which we are not here concerned. Sub-atmospheric waste and degradation implies of necessity previous elevation; and so it would seem impossible to deny that before the close of the Palæozoic Period extensive portions of the primordial archæan floor were worked up to the surface. And if we are justified in making deductions from the conclusions arrived at in Section ii. of this work, there really does not seem to be any very great difficulty in accounting for the non-rigid character required for the metataxic changes wrought in those rock masses to produce foliation, as they were squeezed up from beneath overlying strata, in the *initial* movements which marked out many

* See Journal of the Geological Society, vol. xliv., 'On the Red Rocks of the Devon Coast Section.'

of the great mountain-systems of the globe. When we consider the heat that must have been as yet retained in in those buried masses, owing to the low coefficients of conductivity of the materials which covered them, and the great capacity for heat of water which must have necessarily made the cooling down of oceanic waters a very slow process, and add to this the fact of the long continued saturation of those archæan masses by water so long as they remained below the level of the sea, it can hardly be considered an extravagant or unwarrantable assumption that those rocks continued for the most part, prior to their protrusion in the early formation of mountain-systems, in such a condition as to meet all the requirements of those metataxic changes which appear to have been produced by the mechanical agency whereby their upheaval was effected before the close of the Palæozoic Period.

We must also be prepared to recognize the effect of long-continued pressure upon such rock-materials, in accentuating any incipient tendency to such a taxic order as may have resulted from the circumstances under which these materials were deposited, as the minerals produced by paramorphic change among them assumed a more distinct and definite individuality. This has been already touched upon in Section ii. of this work; and in Section iii. the effect of (hydrostatic) pressure has been recognized as aiding crystallization of nearly all mineral bodies except that of water itself, the peculiar function of which in this respect (that is to say, its liquefaction by pressure) is only one of the many known properties of *burnt hydrogen*, which make it the most wonderful (though the most common) and most important chemical body in the whole economy of nature. If such an incipient tendency existed in the materials of the fundamental schists and gneisses, it can hardly be doubted that long-continued pressure would tend to develope such a structural character. We may parallel such a case by what we know by direct observation of the difference in the degrees of definition of the lamination of a Coal-measure 'clunch,' a Liassic 'shale' and a Tertiary 'laminated clay.'

These considerations are suggested as throwing light upon the fact that the same materials are found in the non-foliated granitic rocks and foliated gneisses, with the frequently observed fan-like arrangement of the latter on a large scale, in the central crystalline axis of such great mountain-ranges as the Alps: and they meet, I venture to think, the case of the rocks in the Cornish peninsula described by Mr. Teall *(op. cit.)*, since these are but parts of the core of a great

mountain system,* which was initiated (as we know from physical evidence) in palæozoic times.

δ. *Metataxic work done by Solar and Lunar Tides.* On thinking over the various mechanical and physical forces which must have acted upon the earth in the early stages of its developement, it has occurred to me that geologists have made too little of the new insight into the past which has been gained by the recent researches of Prof. Charles H. Darwin, as to the cycle of changes that have been going on concomitantly in the length of day and night, the moon's distance from the earth, and the time of its revolution in its orbit. Taking his stand upon the results obtained from such purely mathematical and physical data, Prof. R. S. Ball five years ago gave us such a "Glimpse through the Corridors of Time," as made the more extreme doctrines of the Huttonian school of geologists totter on their pedestals. A general *laissez-faire* sort of acceptance of the views of the more advanced metamorphists consoled some of us perhaps with the notion that it did not matter much whether tides had been greater agents of degradation of denudation and of transport in the remote ages of the Palæozoic Period than in more recent times, since it was assumed that the oldest rocks we know were derived from other still older rocks. As however the argument has developed itself in this work, we have been led to see that on many grounds there are grave reasons for rejecting such a doctrine.

The idea of the work done by solar and lunar tides upon the non-consolidated magma in the archæan and pre-archæan stages of the Earth's evolution has arisen quite independently in my own mind in the course of writing the thesis on which this work is based.†

Taking the general idea of Prof. R. S. Ball as it was published by him, and accepting the general principle, we must demur to one of the results speculatively arrived at by that distinguished astronomer. He applies a dynamical principle quantitatively, and from the data furnished by a very moderate tidal wave of the present ocean arrives at the

* The recent work of Prof. Bonney in South Devon has placed this in a very clear light. See his paper on 'The Geology of the South Devon Coast' (Q.J.G.S. vol. xl.) also the paper by the same author on 'The Older Rocks of Brittany' (*ibid.* August, 1887.)

† In the course of the passage of this work in its present form through the press I have discovered that the idea of a mobile magma was thrown out more than twenty years ago by Macfarlane, 'Origin of the Eruptive and Primary Rocks,' pp. 62, 63. See also Naumann, 'Lehrbuch der Geognosie,' quoted by that author, *(loc. cit.)*

conclusion that the tidal wave must have been so many millions of years ago so many hundreds of feet high. It would be perhaps impossible to challenge this deduction, could we only assume *that the quantity of water condensed on the globe was the same as at present.* This manifestly by his own showing we have no right to do; and so we must call upon Prof. Ball to take this very large qualifying circumstance into account. When however this is done, we can still leave him a wide berth for the dynamic action of the oceanic tides of the palæozoic and later pre-Cambrian ages. But as geologists, and especially in connection with our present subject, we begin to suspect that Prof. Ball has only told us half the story. He tells us of the enormous action of the Moon upon terrestrial waters, when that orb revolved near the Earth at such a proportionately greater velocity as is required by Kepler's law of "equal spaces in equal times." We go further back, and ask what was the effect of the Moon's attraction both on the magma and on the dense atmosphere which enveloped it, at a period still more remote from the present; at a time when the surface of the globe was too hot for the condensation of oceanic waters, or for any other water than such as was entangled mechanically among, or combined chemically with, the mineral constituents of the molten or semi-molten lithosphere; for such he conceives to have been the condition of things at the surface of our planet for some time after the 'Moon's birth.' And upon the back of this comes another question, as to what was the work done by the solar tides, of the existence of which he tells us, in the non-consolidated crust, even at a time anterior to that portentous event. May it not be suggested that, when the answers to these questions come to be carefully thought out, it may be seen that *the action of those primæval tides must have resulted in extensive metataxic change in the non-consolidated materials of those fundamental rocks, and that in this we have at last found a clue to the mystery of their foliation?* In such an unequally viscous mass there would be tension, contortion, and shearing to any extent during the tidal pulsations which the magma was suffering. The more yielding portions would participate more readily in the undulations, and would therefore shear over other less yielding portions with which they were in contact. Portions already solidified, or nearly so, by segregation or otherwise, as time went on, would by their *vis inertiæ* present obstacles around which a fluxion structure would develope itself in the contiguous portions of the yielding magma, giving us perhaps in some cases 'Augengneiss.' The local tension of parts of the viscous lithosphere especially near the crests of the waves,

would imply stretching and consequent lowering of temperature, a circumstance favourable to local solidification. Who shall say that in the later and feebler struggles of this kind, as secular cooling went on and the magma approached nearer and nearer to the conditions required for consolidation, some of these tidal waves may not have become *in situ* sufficiently rigid to outline some of the earliest lines of elevation? *

The term 'Augengneiss' has a definite use in German petrography, and is properly used to denote those gneisses "in which single large felspar-masses (orthoclase), in form ranging from the blunt lenticular to the approximately spherical, occur, segregated from the matrix of the rock which possesses a schistose (schieferig) or coarse grained (flaserig) structure, and around which the mica-laminæ adhere." (Credner, *op. cit.*, 6th ed., p. 101.) Such a structure could easily be developed in parts of a slowly-cooling siliceous magma moving differentially under pressure (shearing); and the difficulty of conceiving the same segregation-structure as developed by the deformation of a solid rock-mass under pressure seems to leave us very good grounds, so far as this point goes, for preferring a primitive and general plutonic origin for such a structure to dynamic action resulting from pressure applied on a 'regional' scale. Of course in either case dynamical action occurs: the *crux* of the question is as to the *origin* of such dynamical action and the *conditions* under which it acted. The introduction of the qualifying word 'dynamic' into the discussion of the structures of the (so called) metamorphic rocks, as between the 'mechanical' school on the one side and the plutonists on the other, leads to nothing but ambiguity.

The habit of speaking of "eyes" in a crystalline rock has however with some writers in this country transgressed considerably the limits of the above-quoted definition of Augen-gneiss, till at last it has become difficult to say that with them even a sheared portion of an intrusive granite, with its included and modified fragments from the adjacent rocks, may not be comprehended under the term.

Orthoclase is probably the embryonic silicate of the terrestrial lithosphere; and it is conceivable that in parts of the primordial magma which were under little or no strain from differential movement of any kind, such felspathic nuclei as are seen in Augen-gneiss proper may have formed by simple segregation, that segregation being arrested when differential movements, such as those which a slow-moving tidal wave would require, began to develope that metataxic process, which, with the different rates of solidifying of the minerals of the residual magma, resulted in the structure which we call 'foliation.' Again, if we extend the term 'Augen-gneiss' so as to include those gneisses in which lenticular masses of a more basic rock (*e.g.* amphibolite) occur,† and admit (as we must on general grounds) the possibility of local and accidental concentration of the heavy bases in the primitive magma, and bear in mind the greater rapidity with which highly basic slags are known to solidify, we can explain the more pronounced foliation of the acid matrix of the gneiss, by the theory here put forward, in such a way as to involve no mystery, and to conflict with no known laws of nature or observed phenomena.

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* Ridges thus formed would have been planed off by subsequent oceanic tidal action. May we not however here find a clue to the rhyolitic-structure frequently observed in the included felsitic fragments of the Cambrian conglomerates? (See App. ii, Note O.)

† *e.g.* in the grey gneiss of Elterlein. Credner, *op. cit.*, (6th ed.) Fig. 133.

In such a case as that here cited the feeble foliation of the amphibolite tells us that the mass was undergoing slow differential movement when the individualization of its minerals commenced.*

I would suggest one point more, and that is the possibility that we *may* come ultimately to associate the feeble foliation of the fundamental gneiss, where it has not been interfered with by mountain-building processes, with the earliest *solar* tidal waves, and the more pronounced foliation of the archæan schists with the subsequent *lunar* tidal waves of the magma. Even those appearances which simulate 'false bedding' in them, on which several recent writers have laid considerable stress, and the not infrequent interstratification of gneiss with schists, may perhaps be partly accounted for in this way. The subject is worthy at least, I venture to think, of some attention. In the developement of foliation and its allied structural characters in the way here indicated in the fundamental gneisses and schists, the chief difference between them and similar rocks, in which in some cases perhaps the general direction of the foliation enables us to connect it causally with lateral pressure operating in the mountain-building process, would be that in the former case we should expect to trace effects due to the *pulling-out*, and in the latter effects due to the *squeezing-out* of the mass. From our present point of view petrology would seem to be relieved of the incubus which has been imposed upon it in some quarters, of having to furnish a theory to account for deformation on a 'regional' scale, *quâ* production of foliation by pressure acting upon previously solidified rocks.†

* With a still more liberal use of the term 'Augen-gneiss' we might include in it cases where, as the outer zone of the lithosphere solidified through the dissipation of energy by radiation, the tidal movements still continuing, huge masses, (even 'regional' masses) of the solidified outer 'crust' would *slide over the viscous magma beneath*, as the strain produced by tidal movements in the latter caused huge fractures here and there in the crust. As the magma grew more and more viscous, it is certain that under such circumstances large fragments of the thin crust would be torn away, included in the magma, and transferred in some cases to considerable distances. This seems the most natural explanation of the striking facts observed by Lawson in the Laurentian-gneiss, and described by him in his Essay published by the International Geological Congress, (1888). Facts however such as those described by Lawson are not new to science. Facts of a similar nature were described by Macfarlane more than twenty years ago; see his paper 'On the Geological Formations of Lake Superior,' (*Canadian Naturalist*, May, 1867).

† Then as now the tidal action would vary, (within much wider limits however) with the relative positions of the Sun, the Earth, and the Moon, the maximum effect being produced when the Moon was 'in meridian.' Pfaff (*Allgem. Geol. als. ex. Wiss.*, p. 188, *et seq.*) has discussed the action of tidal

In Sec. ii we have regarded the earth from the point of view of thermal chemistry and physics as passing through a stage in the history of the evolution of its κοσμος out of its original non-differentiated χαος, when such physical conditions (temperature and pressure) prevailed universally at its surface in relation to the fusibility of the different minerals then being deposited to form a 'crust' as now prevail in the glacier-zone of a mountain-range relatively to the liquefaction of ice.

We can recognise a real distinction between granular, vitreous,* and crystalline ice; and the metatropic and metataxic changes by which a glacier is developed through the stages of snow, Firn (névé), and that crystalline-granular form which constitutes 'glacier-ice' (with its more or less slabby stratification and frequently subordinated banded structure with white air-charged and blue air-free laminæ) have been so fully studied during the last quarter of a century by some of our foremost physicists (Helmholtz, Tyndall, Forbes, and others) that we have little difficulty in referring the peculiar structure of glacier-ice to unequal rapidity of motion under pressure of the particles of different parts of the mass of the glacier. To put the matter more definitely: since equal forces must do equal work in equal times,

if w = the work done upon a yielding mass,
t = the tenacity of cohesion of the mass,
and d = the amount of change of form produced by the action of any force p;
then for any value of p we have in unit of time
$$w = d \times t;$$

that is to say d varies inversely as t. As in the glacier two forces are mechanically at work—the constant action of the earth's gravity resulting in a constant strain of the mass downwards, co-operating with the minor movements produced by alternate expansion (by day) and contraction (by night); so in the case of the semi-fluid magma out of which the earliest solid 'crust' was formed, we have in this section suggested the action of two mechanical forces—the constant tug of the moon's attraction tending to produce a tidal wave

movements in the magma upon the earliest rind of the Earth, initiating the first permanent inequalities upon its surface. If the archæan schists (taken as a whole) represent this first-formed rind, their materials as they accumulated *by precipitation from the heavy atmosphere* being bathed through and through with H_2O in a highly-superheated condition, we seem to have at once an explanation both of the frequent recurrence of gneiss (in a subordinate degree) among the schists as a result of tidal movements, and of those lithological characters by which they have misled the Neptunists into regarding them as 'sediments.'

* See App. ii, Note F.

after it, with a consequent constant strain in the direction of the motion of that wave, and a series of minor tides due to the attraction of the more distant sun. In the case of the glacier we certainly have to take into account liquefaction under pressure and regelation; but this we have to do by way of explaining the 'viscous flow' of the mass. The analogy drawn above is between the behaviour of a body such as glacier-ice possessed *de facto* of the yielding property which enables it to 'flow,' and that of a viscous mass of rock-material. The banded structure of both may be explained by reference to the same physical laws.

For the 'banded structure' of fundamental rocks—whether on a large scale or that smaller scale which we see in 'foliation' (in the strictly-limited sense of the latter term)—we thus come to recognise a complex result arising out of the co-operation of a complex series of causes; the complicated metataxic changes being connected with such conditions as—

(*a*) inequality of distribution of thermal energy;
(*b*) variations in the rate of deposition of the original materials;
(*c*) variations in the fusibility and consequent yielding property,* and in the specific gravity of the minerals resulting from primary paramorphic changes;

all these acting concomitantly with a *general dissipation of energy* tending to make the magma approximate nearer and nearer to the resultant condition of a solid 'crust.' If we regard any one great class of foliated rocks—the gneisses, for example—and the great difference in the degrees to which their foliation is accentuated, from that feeble degree of foliation which makes it difficult to distinguish it in a hand-specimen from a normal granite, to such a marked foliation as may be seen in some of the gneiss of the Alps,† we can hardly avoid the conclusion that pressure exerted subsequently by overlying palæozoic strata has contributed its quota in some cases, before the materials of the gneiss were completely solidified.

If we eliminate the large factor of paramorphic change (which is essentially chemical) and confine our attention to

* The behaviour of highly acid slags from the blast-furnace as compared with that of more basic slags from copper and lead furnaces, the former flowing sluggishly and solidifying slowly, while the latter flow quickly and harden suddenly, has a most important bearing upon the question of foliation on a regional scale. This was pointed out more than twenty years ago by Macfarlane. (*Canadian Naturalist, loc. cit.*, 1864).

† In some of the crystalline rocks of the Engadine (*e.g.*) composed of felspar, mica, and quartz, I have observed an unusually distinct foliation.

metatropic (physical) and metataxic (mechanical) changes, it is not difficult to draw a parallel between the process by which we have conceived the vast thickness of crystalline rocks of the primordial 'crust' with their coarsely-banded (slabby) and their finely-banded (foliated) structure to have been developed out of the earth's original nebulous mass and the process whereby a crystalline rock-mass of glacier-ice with its banded structure is known to be derived from the water-vapour suspended in the present atmosphere. To the geologist who refuses to look at phenomena of the class generally called 'geological' in the light of physical and chemical ideas all this must read rather like romance than sober science. It is not for such that it has been written.

§ V. HYPERPHORIC CHANGE.

As a typical process under this head let us consider *dolomitization*. A limestone containing originally but a small percentage of carbonate of magnesia is in course of time so altered in the proportion of its constituents that it becomes more and more dolomitic, and in some instances is at last converted into a *true dolomite*. This must occur in one of two ways; either more and more carbonate of magnesia is deposited within, or carbonate of lime is removed from, the rock-mass. The former process would be accompanied by an increase, the latter by a decrease, in bulk of the rock-mass. Observations in the field afford abundant evidence of decrease of volume as a concomitant of dolomitization. What has the chemistry of the laboratory to say to this? Its answer is twofold:—

(1) Experiment shows that water containing free carbonic acid dissolves a certain amount of carbonate of magnesia, but that this is insignificant as compared with the amount of carbonate of lime which the same quantity of equally carbonated water can dissolve.*

(2) Thermal chemistry leads us to consider the relative *stabilities* of the two carbonates, and reveals to us the fact that carbonate of lime begins to undergo dissociation with only a moderate elevation of temperature (having the least stability of all the carbonates of the alkaline earths), while carbonate of magnesia is distinguished from most artificially-

* See Roth: *Allgem. u. Chem. Geologie*, Bd. I, pp. 70-80.

prepared salts of metals by its very great stability. It only begins to undergo dissociation (under a pressure of one atmosphere) at temperatures above 300° C, and is only attacked very feebly at ordinary temperatures by dilute acids.

Thus chemistry settles in a very definite manner the question as to the *modus operandi* of dolomitization; and its answer is in accord with observations in the field on loss of bulk of the rock-mass.* A further confirmation of this is found in the high percentage of carbonate of lime which springs issuing from dolomitic limestones contain, and the enormous quantity of calcareous travertine which they deposit. This latter fact can hardly fail to force itself upon the attention of an observer, who in the light of chemical ideas will examine the magnificent coast-sections of the magnesian limestone of Durham. It also explains the formation of dolomitic sandstones out of the residual dolomitic sand of the rock-mass of which the Mansfield freestone is a well-marked and well-known instance. It perhaps accounts, as I suggested in a discussion in Section C of the British Association four years ago, for the great quantity of carbonate of lime which is mingled with the fine siliceous Alpine detritus in the Löss of the Upper Rhine country: the derivation of this mineral from the regions of the head-quarters of the Rhine and the dolomitization of the limestones of those regions being thus seen to be concomitant phenomena. The comparative readiness with which carbonate of lime undergoes dissociation enables us to see how heat may act as an important factor in dolomitization within certain limits: lime, one product of dissociation being readily taken up as a soluble hydrate by pure water, and carbonic acid (the other product of dissociation) contributing to the solvent activity of contiguous water within the neighbouring rock.

These principles taken into consideration along with the great tendency of magnesium to form *double salts* seem to throw some light upon the obscure process by which the drusy cavities of so-called "potato-stones" are lined with a deposit of pure dolomite or bitter-spar, in the dolomite rocks of the magnesian limestone of England and in the Rauchwacke of Thüringen and other regions of the continent.

Hyperphoric † change as illustrated by dolomitization includes then all those simple processes in which *the character of the rock is changed by the removal of one or more mineral constituents from, or the introduction of one or more constituents*

* Dwellers on the Magnesian Limestone districts of Durham are from time to time unpleasantly reminded of this by the Erdfälle which occur as a consequence of it, cavernous portions of the rock collapsing into brecciated masses.

† ὑπερφερω.

into, the rock-mass. The active agencies concerned (partly chemical, partly physical) vary in different cases. But it will be seen that hyperphoric change is the essential principle of such minor processes of 'metamorphism' as

(1) The conversion in some instances of a vesicular dolerite into an amygdaloid, especially where the dolerite is intrusive among limestones.

(2) The gradual formation of gigantic masses of nearly pure rock-salt (NaCl) as in the Eastern Alps and in the Stassfurt district. In the latter district the massive saline deposits are known to reach a thickness of 400 metres, of which the lowest 200 metres and more are said to consist of *pure rock salt.* How was this formed? Apparently not by original deposition from concentrated sea-water or mineral-springs, since in all these other salts are invariably present. The chemistry of solubilities seems to help us here. We have only to recollect that the coefficient of solubility of Na Cl in water is only very slightly affected by temperature (increasing only from 34 to 36 % for a rise of temperature from 0° to 30°C) while the coefficients of solubility of the other salts contained in sea-water and in the upper layers of the saline deposits of Stassfurt and other districts, increase rapidly (that of sylvine for example from 30 to 38 % and that of Glauber salt from 5 to nearly 40 %) for an equal elevation of the temperature of the water. The application of these facts as involving hyperphoric change is obvious enough, on the assumption that the temperature of the water in the lower beds is higher than that of the water in the beds above.

(3) One of the most interesting and best understood instances of hyperphoric change is the derivation of the selenite found in clays from pyrites, involving a transfer of the element sulphur. We have in this case—

(i.) introduction of free oxygen in solution in atmospheric water, with a consequent oxidation of the sulphide of iron into a soluble hydrated sulphate of that metal, and a transfer of the mineral thus formed by water;

(ii.) oxidation of the base into a hydrated peroxide, which is precipitated, while the acid radicle is carried away as free sulphuric acid;

(iii.) a direct chemical reaction, $CaCO_3 + H_2SO_4 = CaSO_4 + H_2CO_3$, as soon as the free sulphuric acid comes into contact with carbonate of lime, the sulphate of lime thus formed crystallizing as selenite.

I have observed very fine examples of this in the clays exposed in railway-cuttings near Grantham, and in other places.

As pointed out in the introductory section of this work, changes such as we have here named 'hyperphoric' do not (acting alone) constitute true metamorphism; yet it must be admitted that in many cases, and especially in 'contact-metamorphism,' they play an important part conjointly with other agencies. The allotment of a short space to their consideration seems therefore to be justified.

§ vi. CONTACT-METAMORPHISM.*

This part of our subject has been so thoroughly studied as to need only a brief consideration here.

The phenomena presented by it exhibit the operation of all the four principles discussed in the foregoing sections of this work:

a. Paramorphic change (§ ii);
b. Metatropic change (§ iii);
c. Metataxic change (§ iv);
d. Hyperphoric change (§ v).

There is one consideration however involved in the study of contact-metamorphism, which—so far as I know the literature of the subject—does not appear to have had the attention bestowed upon it which it deserves; that is the *factor of time*, upon which Lyell laid so much stress. We have no right to assume that *all* the results, which we can recognise to-day in any region in which the rocks have undergone the metamorphism which can be traced to the contiguity or proximity of intrusive masses of igneous rock, are traceable *directly* to its action. We have to consider also the possibility of further changes occurring after those forces, which the intrusion of the igneous mass called into action, ceased to operate. This will be seen as we proceed. If we try to work out the probable sequence of changes (in outline) it is not difficult to conceive of them as falling into several distinct stages.

First stage. Direct effects of heat and pressure. From the principle of dissipation of energy it follows that the glowing intrusive mass must continue to impart heat to the neighbouring rocks until equalization of temperature is reached. The glowing mass having to make room for itself must displace the neighbouring rocks; and, as results of this, fracture, crushing, contortion, and shearing must follow. Also by transmission of

* The terms 'exogenous' and 'endogenous' have been used to denote respectively the appearance of new minerals in the adjacent rocks and in the intrusive mass. (See Kalkowsky, *Lithologie*, p. 33).

the pressure cleavage may be induced in rocks of the outer zone of the region affected. The proportion of these will vary in different instances according to the nature and condition of the rocks acted upon; the more rigid rocks suffering more crushing and fracture, those in a more pasty condition suffering more shearing. There will also be some variation in these results in different parts, according to the direction in which the pressure is applied to them. In every case mechanical force is transformed into heat, and this is added to that received by conduction from the glowing intrusive mass, together with that produced by the friction of the intrusive mass against the rocks into which it is intruded. The direct effects of all this would be mainly metataxic and metatropic in their nature. As *metatropic* results we may note such as *vitrification*, *fritting* and *baking*, both of the adjacent rocks and the fragments caught up and enclosed in the molten mass, the results being often most marked in the latter. In many cases they are melted directly into a glass, in others crushed and recemented by a glassy cement. Some are molten wholly or on the exterior into slags, of which latter I have a good example which I obtained some years ago from the ash-cone of the Pulvermaar in the Eifel; others are rendered porcellanic in their hardness, or acquire in some instances a columnar structure (as I have known lumps of fire-clay do in the burning pit-heaps of the coal-country); others again are coloured or bleached (according to the nature of their accessory materials) by baking. According to Lehmann cases occur in which the enclosed masses are (wholly or in part) so completely liquefied as to allow diffusion of their material through the surrounding magma, reacting upon it to change locally its actual composition.* In the Vorder-Eifel and other parts of the Continent fragments of clay-slate and grauwacke have been observed baked on the exterior; fragments of mica-schist, quartz, and gneiss are found covered over with a vitreous crust; fragments of sericite-schist (in the particular locality Naurod near Wiesbaden) have only their layers of sericite and chlorite vitrified. Even the fragments caught up from older igneous masses, through which (or upon which) later flows find their way, are not exempt from this kind of metatropic change; as in the case recorded in the Isle of Ischia, where fragments of an older trachyte-flow are found included in a later flow and converted into slag. Fragments

* A very striking instance of this has been described by Macfarlane in the basic intrusive rocks of the Huronian Series of Lake Superior. In some places the included granite fragments have been so completely dissolved by the basic magma as to convert the rock locally by shearing into a 'siliceous slate rock.' (*Canadian Naturalist*, May, 1867.)

of limestone are converted into marble, but not always wholly so; the larger fragments retaining in some cases an unmetamorphosed core, which gives a direct clue to their parent-rock. As remarkable examples of this we may cite the fragments of Silurian limestone at Escabar in the Pyrenees described some years ago by Zirkel, the fragments of Trias limestone in the augite-porphyry of the Grödnerthal described by Richthofen.

Similar in their nature are the changes produced upon the neighbouring rocks, so far as the action of *dry heat* is concerned. Sandstones are decolorised and often fritted to a glistening enamel-like or porcellanic mass; in other cases where the cement is of a calcareo-argillaceous nature this is melted into a glass; clay and marl are converted into porcellanite or brick, with marked change of colour in many cases; tuffs and phonolites are so far vitrified as to acquire a character resembling that of obsidian; brown-coal is altered into seam-coal or anthracite, and these in other cases into a substance more resembling graphite, while in others (probably under less pressure) the coal is converted into coke,* the various shades of metatropic change of brown-coal into anthracite, carbon-glance, bituminous coal, and black-coal being observed in some cases in the same section through several metres of the mass; a prismatic structure is developed, not only in clays and marls, but even in sandstones, in brown-coal, in seam-coal, in dolomite; limestones are altered into crystalline marble, often with complete effacement of their stratification and even of all traces of their fossils; the finer varieties of grauwacke and its associated shales are converted into hornstone, as in the classical region of the Brocken. Of these metatropic changes by the action of dry heat under pressure, that of the formation of marble has been experimentally verified years ago by G. Rose, and more recently by Richthofen and others.

The amount of metataxic change which the neighbouring rocks may undergo will vary greatly according to the nature of the rock upon which the force is brought to bear; and it would be difficult to deny that along with cleavage as a direct result of pressure in some of the rocks, especially in the more argillaceous, we may have a crude sort of foliation developed in some cases by shearing, *pari passu* with fluxion-structure and a certain degree even of foliation in the intrusive mass

* Marked instances of this are known in the Meissen district; and several years ago I noticed a fine example in the Museum of the Reichsanstalt at Vienna of the local conversion of seam-coal into coke by a basalt dyke which traverses it. These are of course instances of 'destructive distillation.' In Ayrshire, graphite is said to result from the action of intrusive masses of dolerite upon the seams of coal; probably a dissociation-product by contact-action of the hydro-carbons distilled over from the coal.

itself.* In this way the schistosity induced in contact-metamorphism may in part be accounted for. In other cases however the deformation of rocks would proceed rather by way of contortion, or of fracture and crushing, where the rocks were sufficiently rigid; and the importance of this is perhaps chiefly to be seen in the favourable conditions thus brought about for the operation of the agency next to be considered.

Second Stage. Circulation of superheated water between the intrusive mass and the adjacent rocks. In ordinary lava-flows the water included in the outpoured magma, being relieved to a great extent from pressure, escapes principally as steam; but in the case of the plutonic rocks there is no difficulty in understanding the continued action for a lengthened period of time of superheated water, as in M. Daubrée's experiments, in which the water was maintained at a temperature of 400°C for several weeks. This circulation of superheated water in both directions—from the igneous mass into the adjacent rocks, and conversely—must result in bringing its enormously increased solvent power into play upon the mineral constituents of *both sets of rocks*, and especially upon the silicates. Hyperphoric change is thus brought about continuously, so long as the high temperature is maintained and there remain in the original rocks minerals of lower degrees of stability (under such conditions) capable of being taken up by the water and of reacting upon one another to produce minerals of a higher degree of stability. This kind of transfer of material would seem to take place (from foregoing considerations) most extensively by way of solutions of the alkaline silicates, though not confined to these. The temperature of the glowing mass being higher than that of the neighbouring rocks, the solvent action of superheated water must no doubt be exerted in the highest degree as a general rule upon the minerals of the igneous mass; but it can hardly be doubted—though this does not appear to have been very generally recognized—that there is also a converse and subsidiary transfer of mineral matter from the adjacent rocks into the more superficial portions of the igneous mass, resulting in a certain amount of paramorphic change.† This will be seen

* Dr. Barrois ('Modifications et Transformations des Granulites du Morbihan'; *Ann. de la Soc. Geol. du Nord*, T. xv., p. 1, *et seq.*) has described such schistosity along the zone of contact of the granulites with the Cambrian slates. As examples of the same action of shearing in basic rocks, those occurring in the dolerites of the Huronian described by Macfarlane (*op. cit.*) as 'greenstone slate,' and those more recently described by the officers of the G. Survey of Scotland, (Q.J.G.S., vol. xliv., pp. 391-5.) may be cited.

† We must look in all such cases beyond the mere mechanical circulation of water to the action of aqueous diffusion; the tendency, that is to say, of any

to throw some light upon those cases in which it has been alleged that a gradual transition from plutonic into clastic rocks has been observed. No clearer case of this kind of reaction (through the medium of superheated water) of the original sedimentary rock upon the intrusive igneous rock has perhaps ever been noted than that described by Von Cotta as observed by him in the year 1849, in the marble-quarries above Predazzo, where the junction of the granite with the limestone (altered into marble along the zone of contact) is extremely sharp and well defined. Apophytic processes of the granite-mass have been injected in places into the dolomitic limestone, and as these (in one instance at least) are traced further and further from the granite *massif* they become more and more talcose, and ultimately pass into a true serpentine dyke in the midst of the limestone.* Here, where the proportion of mass is all in favour of the influence of the limestone by hydro-thermal action upon the granite, we find that action predominating, furnishing an extreme case of alteration of the igneous rock. The same author noted the partial serpentinization of granite injected into serpentine itself at Waldheim in Saxony.

These recorded observations of Von Cotta's are supported by those of Prof. Heim on the quartz-porphyry of Windgälle in the Central Alps, (*Mech. d. Gebirysbildung*, Bd. i, p. 37,) the general description of which by that author (*op. cit.*, p. 36,) would apply to the Bozen porphyries as I know them. The analysis which he has given of the normal rock shows that its chemical composition approximates closely to that of a granite of medium composition with a rather excessive percentage of the alkalies. "In certain modifications [of the porphyry] the ground-mass contains irregular elongated masses (in some cases more than 2cm. long and 1cm. thick) of streaky dark olive-green serpentine. In thin sections it is seen that these serpentine-masses are often inter-plaited (verflochten) at their boundaries in remarkable dendritic forms with the ground-mass. Strips of the ground-mass are often floated as inclusions in the serpentine. . . . . These modifications of the porphyry are quite local." It is not easy to understand how the MgO required for this serpentinization could be introduced otherwise than by the solvent action of water at rather high temperature and pressure circulating between the intrusive mass and the adjacent rocks. This view is certainly in harmony with the statement of Kalkowsky (*Lithologie*, p. 72). "Endogene Contacterscheinungen sind bisher [in quartz-porphyries] nur in geringem Masse beobachtet worden, es gehört dazu namentlich das Feinerwerden des Körnes, das Wegbleiben der Einsprenglinge." The same writer notes (*op. cit.*, p. 65,) tourmaline and (less frequently) andalusite, hornblende, garnet as (probably) endogenous products of contact-metamorphism in granite near the boundaries of the *massif* and in its apophytic extensions.

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substance dissolved in water to diffuse itself equally throughout the mass. In the water-logged condition therefore of such rocks, whatever is taken up in solution from one part must be distributed through the water until it enters into new combinations; thus the nature of the endogenous changes will be affected by the lithological environment of the intrusive mass.

* See App. ii, Note N.

Even the more basic intrusive masses may undergo such endogenous alteration as to develope near their junction-planes such minerals as tourmaline, pleonast, corundum, andalusite, and others, as seen in the recently investigated instance of the diorite intruded into phyllites at Klausen, (Tirol). Kalkowsky, (*op. cit.*, p. 98).

The late lamented Prof. Carvill Lewis has described the alteration of an ultra-basic peridotite (Kimberlite) in South Africa, in proximity to carboniferous shales, with the endogenous formation of secondary minerals including *diamond*. That an important part has been played by the excess of the bases of iron and magnesium set free in the serpentinization of olivine, aided by heat and pressure, in the reduction of the carbonaceous material, can hardly be doubted. (See Brit. Assoc. Reports, 1886-7).

As another instance of the influence of mass in determining the final results, may be mentioned the occurrence (which I have lately observed) of strongly dichroic hornblende near the junction-plane in the fine-grained felsite which is injected into the hornblendic granite of Mount Sorrel, with which I am pretty familiar from field-observations. In such cases of transfer by the agency of aqueous solutions the mass which supplies the material must be commensurate with the time required for bringing about the result, for the simple reason that, as soon as the materials which furnish the 'antecedents' of a chemical reaction fail, the 'consequents' must fail also. It is not until we realise the full meaning of this that we can appreciate rationally the importance of time as a factor in all such changes, or open our minds to the full extent of the cumulative results which extremely dilute aqueous solutions are capable of producing. Compare the statement of the officers of the Scotch Survey as to the absorption of calcareous matter (Q.J.G.S. xliv., p. 410).

With regard to the changes of composition and structure which eruptive rocks undergo in their outer zones (salbandes) in contact with the neighbouring rocks (modifications endomorphiques,') Barrois remarks *(op. cit.*, p. 2*)* that the basic eruptive rocks have principally furnished a great number of examples of these modifications; but the acid rocks do not escape the law, examples having been described by Lossen in the Hartz, by Lehmann in Saxony, and by himself in Finisterre and at Morbihan.

We must be prepared, from what we know of diffusion of bodies by solution in water, to recognize such influence in different and minor degrees. The ideas put forward in § ii (β), as to the influence of salts dissolved in sea-water upon submarine lava-flows, in promoting secondary paramorphic changes, apply also *(mutatis mutandis)* here. It is impossible then to deny that many and various phases of secondary paramorphism may result in the outer zones of an intrusive igneous mass; and when this is recognized, along with the crude foliation often observed in such zones near the plane of junction as the result of metataxic change in the unconsolidated magma,—not only bases of the alkalies alkaline earths and magnesia, but even alumina* and heavier metallic bases being no doubt in some instances introduced to a slight extent,—we have got a long way towards the explanation of those observed phenomena, which have led many observers to argue in favour of such extreme views of regional metamorphism as would

* It is as a sulphate or a hydrous chloride that Al is mainly capable of transfer in solution. (cf. Roth.: *op. cit.*, p. 112).

persuade us that we may find in many districts all degrees of metamorphic change, proceeding gradually from the unaltered clastic sedimentary rocks at one end of the series, to normal granite (as so much boiled-down sandstone) at the other.* If this is admitted, there seems to be a poor foundation in fact left for extreme views of regional metamorphism; and as we have before disposed of the argument from the presence of clastic materials in crystalline rocks (result of crushing) this beautifully-elaborated but empirical theory tumbles down like a house built of cards.

The formation of exogenous 'contact-minerals' in the rocks adjacent to the intrusive mass by reactions resulting from the hyperphoric introduction into them of mineral-matter in solution from the igneous mass have been long and carefully studied. Among these are to be noted especially the introduction of silicates in solution to form in limestones as they are metamorphosed into marble, garnet, vesuvian, wollastonite, skapolite (wernerite), prehnite, epidote, minerals containing a large percentage of lime; occasionally others containing a smaller percentage of lime, such as hornblende, tremolite; spinell† (in which MgO takes the place of CaO), titanite, fluorspar, and some micas. The Predazzo region has long been renowned among continental petrologists for the facilities it offers for the study of such contact-minerals and their derivative relation to the limestones and the igneous masses intruded into them. Near Monzoni a pretty general sequence of zones has been worked out, characterized in succession by *garnet, augite, serpentine, brucite,*‡ as results of mineral-changes

* See in particular Prof. Green's idealized sketch, *Physical Geology*, p. 453. There is a dangerous fascination about such graphic expressions of general ideas: we are liable to be misled by their very attractiveness and apparently to think that as it looks as if it were so, it must be so; and thus the critical faculty is in danger of having its edge taken of. Take again the concrete instance brought forward by Mr. Green (fig. 126, p. 401). What is there in it that cannot be explained as the obtrusion aud overfolding of a part of the archæan crust together with the oldest Cambrian strata even to the 'bedding' of the 'crystalline' rocks? † See App. ii, Note Q.

‡ A point of considerable theoretical interest seems to receive illustration from this fact. Taking these four minerals in the order in which they are enumerated we seem to have a definite order of variation in the genesis of the exogenous minerals, those portions of the metamorphosed zone which are in immediate contact with the augite-syenite having been presumably raised to the highest temperature, thus:

1. *Garnet* (presumably) $Ca_3 Al_2 Si_3 O_{12}$, gives bases: $SiO_2 = 4 : 3$.
2. *Augite* (presumably) $Ca\ Mg\ Si_2 O_6$, gives bases: $SiO_2 = 2 : 2$.
3. *Serpentine*, $H_4 Mg_3 Si_2 O_9$, gives bases: $SiO_2 = 5 : 2$.
4. *Brucite*, $Mg(OH)_2$.

With the exception of garnet, in which some of the $Al_2 O_3$ may be present as aluminate (cf. App. ii, Note Q), we note a general diminution in the proportion of SiO_2 taken up, with a corresponding increase of readiness to take up 'water of constitution,' with lowering of temperature.

effected by silicates introduced in solution from the intruded augite-syenite. The formation of garnet in this way is a matter of observation in numerous localities. In southern Norway other contact-minerals have been carefully studied, portions of the Silurian strata having been, as it appears, metamorphosed completely by the granite and its numerous apophytic intrusions. Not only are limestones converted into marble, but calcareous slates also into a calcareous hornstone or a crystalline schistose rock, some layers of which contain garnet and epidote, while gneissic mixtures of mica, pyroxene, quartz and felspar, have been developed. Even the cement-stones are described as converted partly into garnet and vesuvian, and graptolite-shale into chiastolite-slate. In many cases the organic remains of the original rock are found hard by the newly-formed minerals. If these southern Norwegian phenomena can be thus explained as the result of contact-metamorphism, it is difficult to see how they can be quoted in favour of extreme theories of regional metamorphism.

Very noteworthy are the results produced by the intrusion of igneous masses into rocks which have been converted by previous metataxic change into slates. Here we have often a regular sequence of phenomena, for exact knowledge of which we are indebted largely to Rosenbusch. The normal and progressive order of change observed is, according to that distinguished petrologist, (1) the appearance of small knotty concretionary bodies in the generally unaltered slates; (2) these increase in number and quality as we approach nearer to the granite or other central intrusive mass, while *pari passu* with this the slate becomes more intensely crystalline, until it assumes almost a schistose character; (3) as this schistosity is more and more developed the knotty appearances diminish and ultimately disappear altogether, the slates passing into a thoroughly crystalline rock with a complete schistose structure at the zone of contact. These phases of structure occupy in some cases altogether zones of rock whose thickness is measured by hundreds of metres. Rosenbusch has described cases in the Vosges, in which he distinguishes—

a. the zone of Knotenthonschiefer;
b. the zone of Knotenglimmerschiefer;
c. the zone of hornstone and andalusite-hornstone.

Similiar results of contact-metamorphism have been worked out in the Erzgebirge by officers of the Saxon Survey, where a similar increase of intensity of alteration towards the granite is seen in the phyllites, quite independent of the original phyllitic structure, furnishing well-marked zones of

a. Fruchtschiefer with unaltered ground-mass of slate;
b. Fruchtschiefer with a crystalline ground-mass of slate;
c. schistose micaceous rock;
d. andalusite-mica rock.

F. E. Müller has described similar metamorphic stages in the neighbourhood of granite in Thüringen. Even gneiss and mica-schists undergo contact-metamorphism, with the developement in them of such new minerals as sillimanite, andalusite, and tourmaline.

The developement of felspar, sillimanite and biotite in the Lower Silurian sericitic sandstones of Morbihan in Brittany has been described by Barrois, as also an outer contact-zone of biotite-bearing quartzite.

The comparative and analytical studies of Rosenbusch and others have led to the conclusion that in the contact metamorphism of some slates, in the zone near the granite, the change consists in a molecular re-arrangement of the original constituents of the slates with a simultaneous taking-up of water of constitution (metataxic and metatropic changes) rather than in the introduction of new mineral-substances. In the exogenous developement however of such minerals as tourmaline, as described by Allport *(op. supra cit.)*, new mineral-matter must have been introduced.

For the facts cited above I am largely indebted to the latest edition of Credner's Geology. Many others may be gleaned from works and memoirs published in this country.

Third stage. Changes following upon the cooling of the igneous intrusive mass. So far as my reading goes, these have received little consideration. Yet a little reflection will show that some further changes may occur in many cases. The igneous mass having in the course of its intrusion made a space for itself to occupy continues to occupy that space so long as its high temperature is maintained; but as its temperature gradually falls it suffers loss of bulk. The necessary result of this is either (*a*) a considerable widening of the junction-plane, or (*b*) a settling-down of the displaced rocks to fill the space left unoccupied. The latter may leave its mark in *Ausweichungsclivage** in the adjoining rocks, while a certain amount of contortion or fracture with perhaps faulting on no very large scale may occur in the rocks nearer the junction-plane. If these results do not at once follow, the junction-plane is meanwhile widened. In cases where the igneous mass has become welded to the adjacent rock, the latter may part asunder at places of greatest structural

* 'Ausweichung' *(lit.)* lateral yielding. See Heim; *Mech. der Gebirgsbildung.* Bonney's term 'strain-slip' is a very good English equivalent.

weakness.* In either case it is easy to see that increased facilities are given for the access of water from above, bringing with it in solution either atmospheric gases or mineral substances taken up in its passage through superjacent rock-masses. The access of this water must lead in many cases to a series of secondary paramorphic changes both in the surface portions of the cooled igneous mass and still more in the adjacent (and in some instances partly disintegrated) rocks of the already metamorphosed zone. The water itself would aid in the further disintegration of the rocks to which it thus found freer access. It is possible that much of the water-of-constitution noticed by Rosenbusch may have been taken up under such conditions, which are more favourable to it than conditions of high temperature. A certain loosening of the structure of the rock from the causes here indicated may, along with the freer access of water, help to explain the 'knotty' texture developed in them. If such changes went on for a length of time, a great deal of work might be done to prepare the way for such metatropic and metataxic changes as would result in developing to a greater degree the schistosity of the rocks of the contact-zone under the influence of pressure exerted at a later period in great earth-movements upon the region.† Many of the phenomena presented in regions of contact-metamorphism may thus be found susceptible of a more rational explanation than the hypothesis of direct deformation of a solid rigid rock by pressure. The work done by pressure would in such a case consist rather in re-forming than in deforming the rock.

The ultimate result would be to give us a pseudo-schist, by the welding and shearing under pressure of the loose materials in which long-continued hydrochemical action had previously wrought such secondary paramorphic changes as have been by some writers attributed to the pressure itself. In fact the result would be much the same as the action of pressure and shearing upon a mass of fault-débris, where the adjoining rocks could furnish the required mineral constituents, the true history probably of some pseudo-schists which take a dyke-like form.

* This I believe to be the explanation of the peculiar marking of many marbles, such as the Devonian red marbles with their included corals. The cracks (filled subsequently with secondary calcite) are so disposed as to suggest the rock having been torn asunder much in the same way as the marginal transverse crevasses of a glacier are formed, where the margin of the glacier is held by the rock of the valley-side, while the more central portions of the mass move onwards. I am also inclined to think that the peculiar structure of the 'Marbre de Plestin' of Barrois, (*Am. S. G. du Nord* T. xv, Pl. iv.) may be explained in this way.

† There is a third result possible. The contraction due to the cooling of the outer and more elevated parts of the intrusive mass might, by the pressure of that mass and the adjacent rocks, cause a welling-up of portions of the still fluid magma beneath. Such may be the possible explanation (as its relation to the granitic massif would suggest) of the occurrence of aplite so frequently near the junction in the intrusive rocks of Brittany. (See Barrois, *op. cit.*)

Prof. Boyd Dawkins has lately drawn my attention to some interesting examples (in the Museum of Owens College) of mineral-change in the felspar of the granite contiguous to the principal lode of the Foxdale mine in the Isle of Man, which he has investigated. Near the lode, on the side on which a vein of spathic iron-ore occurs, a good deal of the felspar has acquired a green colour. Under the microscope the plagioclase appears to be quite fresh and unaltered, while the orthoclase has suffered two different modifications; (1) ordinary kaolinization with no green colouration and the separation-out of much SiO_2, (2) alteration to a green mineral, in which the separation-out of SiO_2 appears to have taken place much more sparingly. These facts seemed to me to suggest that traces of iron-salts had been diffused in solution through the granite-zone on that side, and that silicate of iron had been formed by an ordinary chemical reaction between the salts of iron and silicate of potassium of the orthoclase, the stronger acid of the iron-salt taking up the potassium, which thus was carried away in solution, with a simultaneous combination of iron and silica. An analysis of some of this green mineral, which I made in the chemical laboratory of Owens College, showed that this was indeed the case. Secondary (endogenous) paramorphism has occurred here apparently independently of pressure and high temperature.

Many examples of metamorphism in regions contiguous to junction-planes might be cited to which the operation of the causes tending to change in this third stage would seem to apply. For the British area I shall content myself here with reference to one in the Cornish peninsula lately described by so careful a petrologist as Mr. Teall,* and to the published papers of Mr. S. Allport.

After a careful study of Mr. Teall's admirable paper I must say that the facts which he has described seem to me to point very strongly to the deformation of the mineral structure of the metamorphosed portions of the igneous rock by free access of water along the junction-plane, and perhaps through shrinkage-cracks—followed by a re-formation of the rock—of those parts less deformed into a *flaser-gabbro* and of those more deformed into a *gabbro-schist*—by the subsequent action of the pressure and shearing which accompanied those great earth-movements of which the district seems to furnish abundant evidence. The nature of the mineral-changes indicated,—*e.g.*, the diminution in the percentage of silica and the taking-up of water-of-constitution, together with a slight increase of percentages of some of the accessory mineral-ingredients (see Rammelsberg's analyses) in the alteration of labradorite into saussurite—seem to point to the action of water; while the fact that some parts of the rock are stated to be 'brecciated' while other parts are foliated, seems to illustrate the different ways in which the undeformed and the deformed portions of the rock were affected when pressure acted subsequently upon them. When these things are considered, the case of the Lizard gabbros

* "The Metamorphism of the Lizard Gabbros," *Geol. Mag.*, November, 1886.

scarcely seems to give such support to the theory of 'regional metamorphism' as the accomplished author of the paper quoted appears to imagine.

With this we may compare the most instructive instance of the quartz-porphyry of Windgälle in the Central Alps described by Prof. Heim (*Mech. der Gebirgsbildung*, Bd. i, pp. 34-39). At the junction of the pre-carboniferous intrusive porphyry with the older Verrucano-strata there occurs locally a rock of such a character that it may be regarded petrographically as a transition-rock ('Uebergangsgestein') between the two. "The clear grey ground-mass contains still small rose-red felspar crystals and pellucid quartz-grains as the porphyry does, but they are manifestly crushed, and *sericite* occurs in considerable proportion. A definite boundary (Gesteinsgrenze) in the direction of the slaty rocks, described as of Verrucano character, is not to be found." Prof. Heim records his own impression that crushing (Quetschung) has driven the petrographical limit between the two rock series some distance (ein Stück weit) into the originally massive porphyry; and he rejects the notion that either series is a metamorphosed representative of the other. I would suggest that in this instance also we may have the combined results of two distinct processes,—pressure operating during the later mountain-building movements (of which such overwhelming evidence is furnished) upon the intermingled materials produced along the junction-plane in the disintegration of the rocks of both series in this third and latest stage of contact-metamorphism, which had resulted from the freer access of water, and of which the incipient stage is represented by the kaolinization of the outer crust of the intrusive mass. (p. 35.)

Lehmann (*Alt-Kryst. Schiefer-gesteine*, Taf. v. vi.) shows us how pressure produces a metataxic change (not uncommon) in both granitic and clay-slate materials—whether fragments of slate enclosed in the granite, or granite injected into the slate—inducing cleavage in the slate-fragments and converting the granite locally into 'phyllite-gneiss,' without (except in one doubtful case, Taf. v. fig. 3) inducing any paramorphic change of either rock, or even obliterating in the slightest degree the junction of the two. On Taf. v. (fig. 2) mechanical deformation of the quartz is to be noted as apparently the main factor in bringing on the phyllitic structure. Here again it seems not at all unlikely that the quartz, having been perhaps in a hydrated condition, has passed through the 'critical state' (*supra*, pages 48-49.) Taf. xx. of the same work exhibits a fine section of gabbro, in which the flaser-structure and even a 'dünn-schiefrige' structure are seen to be so related to the junction-plane and to the shrinkage-cracks that the explanation suggested in the text of this work (pp. 82-83) seems directly applicable to them.

To the cases cited here may be added the following remarkable instances from Allport's masterly papers:

1. The fact, which he mentions and shows in an illustration (Q.J.G.S. vol. xxxii. fig. 4), of the crystalline tourmaline, in the tourmaline-schists in contiguity with the granite, adapting itself to the form of the previously crystallized quartz, seems to suggest crystallization of both in the 'dry way,' the melting-point of tourmaline being well within the range of furnace and blow-pipe temperatures. (Rammelsberg, *op. cit.*, p. 538).

2. What resembles a 'Fleckschiefer' is described by Allport, (*ibid.* fig. 3) as occurring in the slate about 15 feet from the granite, "the granular parts consisting of numerous flakes of mica thickly crowded together, and somewhat elongated in the direction of the [incipient] foliation," (pp. 409, 10). The observation on page 408 of the same paper, that "decided foliation is restricted to the immediate vicinity of the granite, and appears to replace the original lamination of the fine sedimentary material" is of some importance, as suggesting the necessity for caution in drawing inferences from observations in cases in which a continuous section is not seen.

3. Allport's observations on the phenomena of contact-metamorphism *(Ibid.* pp. 407-10*)* seem to run fairly parallel with those of such foreign workers as are cited in this work, (pp. 81-82). He notes the developement of new minerals (quartz, mica, tourmaline) in the contact-zone of the slates, and the conversion of previously-cleaved clay-slate into a crystalline foliated rock (mica-schist, tourmaline-schist), the arrangement of the new minerals appearing to follow the original lamination of the fine sedimentary material. The intruded granite is also schorlaceous. He also describes a case of developement of crystals of orthoclase felspar, as well as quartz and mica, in the altered Silurian slates of Wexford, in immediate contiguity with the granite *(Ibid.* fig. ii*)*, thus giving us a gneiss as an alteration-product. Yet in spite of such local phases of metamorphism in the contact-zone, the distinction between the granite and the metamorphosed rocks seems to him absolute and incontestable.

4. He notes *(Ibid,* p. 412*)* that outside the contact-zone of schists, "indurated slates traversed by quartz-veins form a wide zone, and beyond these the rock is ordinary clay-slate." With this we may compare Lehmann's example of Knotenschiefer *(Altkr. Gest.* Tafel xxviii, fig. 2*)* which that author describes as showing "the splitting of the original slate visibly into small bits, between which quartz and biotite have separated-out, the secondary quartz (Neubildungen) forming in places wider quartz-layers." All this, in the one case and in the other, looks very like the kind of work we should expect to be done in the *third stage* of contact-metamorphism (*supra*, pp. 82-83). The alteration described by Allport *(loc. cit.)* of crystalline minerals into 'chloritic pseudomorphs' consisting of a green substance which exhibits no bright colours, must also, I think, be assigned to the same stage.

The alleged metamorphic origin of granite. Prestwich summarizes the evidence in favour of this doctrine *(Geology,* vol. i. p. 429*)* as follows:

(1) The later crystallization of the quartz, though quartz has a higher fusion-temperature than the other mineral-constituents of granite.

(2) The lower density of the varieties obtained by artificial fusion of quartz, as compared with the density of the quartz of granite.

These are old arguments, but the reiteration of them by so eminent an authority at this time of day demands some attention. What do they amount to?

(i.) Crystalline quartz certainly fuses at a higher temperature (that of the oxy-hydrogen blow-pipe) than orthoclase and mica do, since they fuse at ordinary blow-pipe temperature. But here we are dealing with *perfectly anhydrous* $Si\ O_2$. Given traces of $H_2\ O$ in the original magma, as crystallization of the anhydrous orthoclase and mica proceeded, the $H_2\ O$ would be more and more concentrated in the residual $Si\ O_2$, which is simply the excess of $Si\ O_2$ over the proportion required as the equivalent proportion for the bases present. The concentration of $H_2\ O$ in this residual SiO_2 would make it more and more hydrated with a consequent *lowering of its fusion-temperature* (as my experiments given in App. i. seem to show). This water by molecular separation from the $Si\ O_2$ would collect in minute bubbles (the true origin perhaps of the minute bubbles included in the cavities); and with this *dehydration of the* $Si\ O_2$, together with the removal by it in solution of traces of *fluxing-materials* (such as Sorby observed in some of the fluid-inclusions of the quartz of granite), crystallization of the $Si\ O_2$ would advance.

(ii.) The argument from relative densities is fallacious, since the *pressure conditions*, under which the experimental evidence referred to is obtained, are not the same as those which obtained in the crystallization of the quartz found in granite.

In considering this point we have a further question to ask in connexion with the work referred to in the foot-note on p. 80. What right has the author to assert generally, [with reference to fig. 231, on page 669 of his work] that "the rock [granite] shades off invisibly into foliated schists, and these

melt away in the unaltered rock."† Rather extensive observation of the succession to be seen in actual mountain-chains is adverse to such a general statement; and Heim's sections across the Alps *(Mechanismus der Gebirgsbildung)*, to which he refers, can scarcely be said to bear it out. Nothing can be more decisive against such a view than the results of the work of Allport, "On the Rocks surrounding the Land's End Mass of Granite" (Q.J.G.S., vol. xxxii.), and that contained in the more recent papers by Dr. Callaway, (i.) "On the Granitic and Schistose Rocks of Northern Donegal," (Q.J.G.S., vol. xli.); (ii.) "On the Alleged Conversion of Crystalline Schists into Igneous Rocks in County Galway," (Q.J.G.S., vol. xliii.); which have disposed of the views maintained in some quarters as to the metamorphic origin of the granite in those districts, by establishing its intrusive origin. Thus in one instance after another that view is found on closer examination to be based on insufficiency of observation. Most important in this connexion is the work of Dr. Ch. Barrois on the 'Modifications et Transformations des Granulites du Morbihan (Granites à 2 micas),' *Annales de la Société géologique du Nord*, t. xv.

GENERAL REMARKS.

Much of the obscurity which has hitherto hung over the subject of 'Metamorphism' has been more in the vague and indefinite—not to say at times shifty—use of the terms employed in discussing the phenomena, rather than in the phenomena themselves.* It ought to be possible to have as sharply-defined a nomenclature in physical geology as in biology or chemistry: indeed, without such an *armamentarium* it is not easy to recognise its claim to a position among the exact sciences; it must continue to hold the secondary position of a mere descriptive study, such as that which "Natural History" occupies in comparison with modern Biology. Some limitations for example must be imposed upon the use of those much-abused terms 'schist' and 'slate'; and there is no better definition of them perhaps than that which Prof. Jukes laid down years ago based on Sedgwick's definition of the terms *lamination*, *cleavage*, and *foliation*. Of the German word 'Schiefer'† there seems to be an almost unlimited variety of uses; but for my part the difficulty is not such a real one as that of the word 'schist,' since the use of the adjectival form 'schieferig' on the one hand, and on the

* The high-sounding term 'metamorphism' (with its cognate forms) has served too often as a sort of ball-and-socket joint, upon which the logical lever may be made to turn any way you please according to the exigencies of the argument. It would be a gain to *Geological Science* if this group of words were to drop out of the nomenclature altogether.

† Nothing can be more treacherous than this word to a reader possessed of only a slight acquaintance with the German language. Without very careful reading a German writer may easily be understood to speak of schists when all the time he means slates or even shales.

other the readiness with which the German language lends itself to word-building, makes it pretty clear generally to a careful reader what a writer intends, whether *(e.g.)* he is speaking of a true schist, of a phyllite, of a slate, or the purely mechanically-imposed structural features of a laminated sandstone or shale. Extensive observation of the last-mentioned varieties of rock in this country and in Germany makes it difficult to deny that in some of these the coating of micaceous spangles on the planes of lamination may have resulted in some instances from subsequent change of some kind by the action of infiltrating waters; yet we should never dream of calling such a rock a 'schist.' Even when such laminated sandstones have undergone such an amount of baking in contact-metamorphism as to cause an incipient interfusion of the mica and the other materials of the rock, while the clastic structure of the rock remains conspicuous, it can only be by an abuse of the term that such a rock can be called a 'schist.' Mere etymology is in this case (as in many another in exact science) worse than worthless as a guide. No more are we justified in applying the term 'schist' or 'schistosity' to the peripheral laminated structure developed in the weathering of some of the dolomitic concretionary mudstones of the Durham coast; or in the weathering of blocks of basalt when acted upon by the humus-acids of the soil or exposed to the alternations of atmospheric conditions (as in the famous Käse-Keller of Bertrich), or exposed to the alternate action of sea-water and air in the parts contiguous to the shrinkage-cracks of an eruptive dyke which intersects a coast-line, excellent examples of which may be seen in the coast-sections of Northumberland.

On the one hand there is often great difficulty in drawing the line between granite and gneiss; and on the other there is even greater difficulty in making a distinction between the schists and mere phyllites. This difficulty is increased by the frequent appearance of 'sheen surfaces' (to which Prof. Bonney has lately drawn attention), so commonly met with for example on the original planes of lamination of the meta-taxically-altered argillaceous shales of North Devon and the Rhine country. This sheen surface is different from the phenomenon of *'slickenside'*—a mere film on the surface of the laminæ—and may be perhaps referred to the freer access of water to the original lamination-planes than to the interior of the laminæ. The proposal of Prof. Geikie (*Text-Book of Geology*, p. 121) to restrict the term 'argillaceous schists' to these is worthy of attention; yet we feel some dissatisfaction in applying the term schist to these rocks at all. Moreover it may be, I believe, observed in some of the oldest

Alpine schists as an accessory and subsequently-induced character.* Even greater difficulty seems to exist in applying the term schist to crunched and baked masses of altered sandstone with highly 'slickensided' vitrified surfaces, such as is included in the intrusive mass in one place in Charnwood Forest; † similar examples of which on a smaller scale I have observed as included fragments along the junction of the granite with the sandstones of Cretaceous age in Saxon Switzerland. Again in the Alps, in the higher Swiss valleys and the deep gorges cut down through the Triassic limestones of the Semmering Alps in the extreme east, one comes across great masses of rock often intersected by veins of secondary quartz and containing within them, scattered—more or less rolled—fragments of vein quartz, around which a pronounced flexure has been given to the *cleavage* (for I do not from macroscopic observation think it is anything more), as if this were the result of considerable shearing accompanying the pressure which has given to the rock its present structure. Blocks of such rock may be seen frequently used as road-posts in the Alps, and in the Inn Valley I have seen them shaped into millstones. My own impression is that they are nothing more than palæozoic 'scree'-materials subsequently altered by pressure crushing and shearing, and that even to these the term 'schist' would be in a strict sense inapplicable; they are in reality slates with very irregular cleavage.

By far the most extensive exposure of such rocks in the Alps is the older Verrucano and Anthracitformation (Heim‡) of the Tödi-Windgällen Group, which that author has worked out so thoroughly. It is in these half-crystalline representatives ('Vertreter,' Heim, *loc. cit.*) of the palæozoic formations in the Central Alps that we find perhaps the nearest approach to a true schist; yet even here we should probably err in ascribing their crystalline character—so far as it goes—to the excessive pressure to which they have been subjected. The crystalline minerals of which they are largely made up, are probably only the finer detritus corresponding to the coarser conglomeratic masses intermingled with them (containing blocks of gneiss, granite, porphyry and red jasper), the whole being probably derived from an archæan Alpine region, as this was subjected in the earliest stages of its elevation to the scouring and disintegrating action of the earlier palæozoic tides which rolled over it; so that at the time of its first emergence as dry land, it had undergone considerable marine abrasion, the removal of the materials of the later archæan rocks from the crest of the region of elevation furnishing the detrital material of the unfossiliferous 'transitional Grauwackenformation,' while its removal from the region of elevation facilitated the extrusion of the yet imperfectly consolidated Central-gneiss series and the developement of the 'fan-structure.' Subsequent hydro-chemical action would account for the secondary minerals (talk, sericite, chlorite) which occur in them; so that when

* Compare Bonney, Pres. Address, 1886, also App. ii, note H.

† I refer to the rock quarried in Bazil (or Brazil) Wood, which I know very well. It may have been once a rather gritty slate.

‡ *Mech. der Gebirgsbildung.* Bd. i., pp. 41—52.

subjected to the enormous pressure and shearing of later mountain-building movements, they would become sufficiently welded together to simulate in some instances the character of a schist or gneiss, some of the rocks even approaching (macroscopically) the character of gneiss in which mica is replaced by anthracite. Heim remarks (*op. cit.* p. 42), "While the Verrucano and rocks of the Casanna-schist Zone are often in hand-specimens [macroscopically] undistinguishable, they differ essentially as a series in that the Verrucano shows no tendency to pass over into amphibolites and massive gneisses or amphibolite-gneisses, as do the rocks of the Casanna Zone." A detailed microscopic examination is, as he remarks (*op. cit.* p. 43), much to be wished. What the result of such an investigation may be would seem to be foreshadowed by Bonney's microscopic researches in the less extensive representatives (Val Orsina conglomerate with its associated grits and slates) of Heim's Verrucano in the Western Alps,* in which he finds no trace of the characteristic foliation and 'metamorphism' of a true gneiss or mica schist. The history of the Central Alpine† Verrucano, which I have suggested above as to me the most probable, applies I believe more or less to the Granulitic Series of Lehmann in Saxony (including the Obermittweida Conglomerates); to the rocks (perhaps) of the Sparagmitétage of the neighbourhood of Bergen in Norway (with which Heim compares the older Verrucano); and to the Huronian Conglomerate near Sudbury in Canada,‡ in which we have perhaps the latest records yet deciphered of rock-building in the pre-Cambrian stage of the history of the earth's lithosphere. It is only by piecing together such fragmentary records from different parts of the world that we can hope ever to arrive at anything like a continuous and consistent view of the gradual changes of physical conditions and their geological operation, which shall bridge over for us the vast gulf that appears to exist between the palæozoic and archæan rocks. We have no more right to assume the existence originally of an universal gulf between the Cambrian and Pre-Cambrian rocks, than between the palæozoic and neozoic series: though great gaps undoubtedly occur, not however in every region marking the same period of the history of the lithosphere; and if Prof. R. D. Irving's paper on the Huronian Group had done nothing else, it would at least be of the greatest value in bringing this idea more prominently into view.§ Such cases as are here cited will serve, it is to be hoped, as the foundations of some of the piers upon which the labours of the geologists of the near future may be able to bridge over the vast abyss. This can however only be done as the scientific geologist frees himself from all empirical theories—all fanciful ideas—of 'regional metamorphism,' which are so dear to the (orthodox) geologist, and approaches the subject with a clear and open mind (enlightened by a first-hand acquaintance with physical and chemical laws), prepared to accept the

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* "A supposed case of metamorphism of an Alpine rock of Carboniferous age." (*Geol. Mag.*, dec. ii, Vol. x., pp. 507–511, 1883.) The more recent published description by Dr. Grubenmann of the Airolo series can scarcely be said to have proved very much. (See App. ii, Note T.)

† In the Eastern Alps the Austrian geologists have worked out a more distinctly differentiated palæozoic series, the 'Silurian' (Von Hauer '*Die Geologie*,' p. 244), having been traced in a continuous zone ('fortlaufender Zug') from Kitzbühel in Tirol to Gloggnitz on the south side of the Vienna basin, the rocks consisting mainly of Thonschiefer, Grauwackensandsteine and dolomitische Kalksteine.

‡ Bonney, Q.J.G.S., February, 1888.

§ The actual transition from the Archæan to the Cambrian exists no doubt beneath the floors of the permanent ocean-basins, the original areas of depression in the contraction of the lithosphere, accentuated by the weight of the water which flowed into them, as aqueous condensation proceeded *pari passu* with the cooling of the lithosphere. (See Heim and Suess, *op. cit.*)

teaching of the evidence (macro- and microscopic) which Nature alone can furnish. The record is undoubtedly but fragmentary; yet the writings of such historians as Mommsen show us how much can be learned in the department of human history when scanty fragments are arranged in their natural order. Bonney's later papers* are simply invaluable as pointing in this direction. His descriptions of the facts are so clear, and his conclusions so logically and tersely stated, that they do not admit of being summarized: they must be read and read again by the earnest student.

Nowhere is the archæan succession better worked out than it has been by the officers of the Saxon Survey in the Erz-gebirge (Credner, *op. cit.* 6th ed., p. 387, fig. 131). There we trace the record of the gradual transition from (i) the *Ur-gneiss* (stage for the most part of dry fusion), through (ii) the *Glimmerschiefer*, (stage of incipient aqueous action under great pressure and at high temperature, perhaps all the water in contact with the lithosphere being either in the condition of super-heated steam or in the liquid-gaseous 'critical-state') to (iii) the *Phyllitformation*, (stage probably of extensive liquefaction of water still highly superheated). As dissipation of energy went on, we should have in some regions the later Cambrian conditions anticipated to some extent [and of course the converse may be true that in some regions the conditions which dominated the later Urschiefer-stage may have lingered on in the Cambrian]; giving us a gradual ascending transition from the highly micaceous rocks (noch mit starkem Glimmerglanz) into the slates (Thonschiefer) above, which in Thüringen, in Vogtland, in the Erzgebirge and in the Fichtelgebirge were first described as Unter-Kambrium (as the Huronian was in America). See Credner, *op. cit.* p. 400. Compare also Dr. Barrois' 'Observations preliminaires sur les roches des environs de Lanmeur'; *Ann. de la Soc. Geol. du. Nord.*, T. xv, p. 238, *et. seq.*

There is some warrant for the supposition that the Cambrian marks the first stage of full oceanic conditions on the globe, the archæan phyllites (Urthonschiefer) being perhaps the products of sedimentation in the earlier stages of aqueous condensation, which must have marked the setting in of the fifth of Zöllner's phases of the existence of worlds.†

On the supposition that the crust had now sufficiently cooled to allow of a general condensation of water upon it, the vast accumulations of the materials of the Cambrian slates, grits and conglomerates can be understood as resulting from the destruction of, and deposition as sedimentary detritus from, the cooled slaggy crust and its volcanic ejectamenta, by the great tidal waves which swept over and levelled down the inequalities of that crust, even though (as some have thought) there may have been no very general elevation of dry land above the ocean-waters in the Cambrian and Silurian periods. And if this were so, it helps us to understand the widespread existence of crystalline allothigenous constituents (mica, &c.) in the Cambrian and Silurian slates. Such a hypothesis seems to account for the observed unconformity between the Archæan and Cambrian rocks, a fact, the significance of which seems to be heightened by the abandonment of the notion of the Cambrian age of the Huronian, and the relegation of it to the Archæan.

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* See especially his three papers in the Q.J.G.S., February, 1888.

† Briefly these may be stated thus:

1st. The nebulous phase (condition of glowing gas).

2nd. The glowing liquid phase (as in fixed stars with constant brilliancy).

3rd. Developement of a solid glowing crust as in fixed stars of variable brilliancy.

4th. Bursting of the thin crust, and extensive eruption of the glowing fluid interior.

5th. Gradual thickening of the solid crust and condensation of water upon it.

The 'meteoric theory' of Prof. Brauns of Halle. ('cette audacieuse hypothese,') may be passed by here. It has received the treatment it merits at the hands of Prof. de Lapparent; see *La Formation de l'Ecorce Terrestre* (pp. 10-14); Brussels 1888. It has certainly furnished an exercise for the mathematician, but it must be recollected that the out-put of the mathematical mill depends upon what is put into it; and the *crux* of the whole question is as to the nature of this.

Considerable confusion has also grown up in the use of the term 'slate,' shales and slates being not infrequently confounded. A rock may of course furnish a very good 'roofing-slate' in the workman's sense of the term (as in the case of the Collyweston and Stonesfield 'slates') and not be a 'slate' at all according to the petrographical use of the word. Where the pseudo-cleavage is nothing more than the accentuation by pressure of the original lamination of a shale, the rock is still a *shale* and nothing more, even though its argillaceous composition is as perfect as that of a true clay-slate, and secondary minerals have been developed on the lamination-planes as separation-products, by long-continued action of percolating water. Whatever the condition in which such a shale is now found, it would be designated *Schieferthon*, in German nomenclature; and thus a clear distinction is drawn between an indurated and slightly altered shale (in the sense just indicated) and clay-slate or *Thonschiefer*.

How far can we attach any meaning to the term 'metamorphism' as applied to the distinctive morphological characters of the archæan rocks?

Passing in review the main points of the evidence, we note:—

1. The alleged evidence of the archæan existence of low forms of *animal-life*. Any one, who, with a fair command of the German language and a moderate acquaintance with Protozoan forms, will form a judgment of the merits of the Eozöon-Controversy from a study at first-hand of Möbius' splendid Monograph, "Der Bau des Eozöon Canadense" (*Palæontographica*, Cassel, 1878), and not from versions thereof which have appeared from time to time in the English language, must admit that the evidence of the organic origin of that structure is very slender indeed. And in the face of the overwhelming physical evidence against the possibility in archæan time of life, as we know it, on this globe, it is not too much perhaps to say that the evidence in favour of Eozöon representing a living form must be made infinitely stronger than it has yet been made in order to establish the case.

For my part I fail to see that there is any better evidence of the organic origin of the so-called Eozöon structure than of such mineral infillings as may be seen in a section of a slab of 'Cotham marble' or in such as those figured in Lehmann's *Altkryst. Schiefergest*, Tafel xvi. fig. 6, which are described by that author in a note as "a layer (in a granulitic rock) in which amphibol has grown inwards from the surface." Compare also Dr. Barrois, *op. cit.* T. xv., pp. 241, 242.

2. As to the existence of *vegetable life*, *graphite* being accounted for (App. i, Note I) as the result of the action of known chemical and physical laws, its presence can no longer be adduced as evidence of the existence of archæan vegetation; nor does 'hydro-carbonaceous' matter in the shales of later pre-Cambrian time necessarily imply the existence of organic matters.*

3. *Iron-ore* (both Fe_2O_3 and Fe_3O_4). Though the former (hydrated) is well known to be precipitated by oxidation of iron-salts in solution in waters containing organic acids, it can equally well be produced by the direct combustion of iron-vapour, while Fe_3O_4 is—as is also perfectly well known in

* See also my paper 'Further Notes on Dissociation by Contact-Action,' *Chem. News*, No. 1505; also that on 'The origin of Graphite in the Archæan Rocks,' B. Assoc. Report, 1888. Compare App. ii, Notes P,T,V.

the chemical laboratory—the direct product of the reducing action of iron at a red heat on superheated steam. The oxides of iron are therefore put out of court as evidence of the existence of archæan vegetation; and with them must go also the iron-carbonates of the Huronian.

4. *Crystalline Limestones*, as has been shown, could have been formed abundantly (both by dry and wet reactions) by ordinary known and demonstrable chemical changes under such gradual changes of thermal conditions as are deducible from the principle of dissipation of energy, and may have been for the most part originally crystalline, and are not necessarily metamorphic.

5. *Quartzites* (including the finer Grauwacke) being seen to represent sometimes simply the excess of SiO_2 which separated out from such reactions as have been considered, and especially by CO_2 in solution where (and when) water was locally condensed on the then-formed crust, there is no necessity for regarding them as altered 'clean sandstones,' or in any sense as *per se* the results of later metamorphic action.*

6 *True Foliation* appears to be quite exceptional in the earlier palæozoic rock (leaving out of account of course cases of contact-metamorphism), and where it does occur in sedimentary rocks of undoubted post-archæan age, seems to amount to no more than 'cleavage-foliation,' producing in such cases mere phyllites, which are common in the later pre-Cambrian rocks.

Are we not justified in saying that the extent of subsequent metamorphism which the archæan rocks have undergone on anything like a general or regional scale is for the most part metataxic; the fact of their having undergone any extensive paramorphic change, beyond such local 'secondary paramorphism' as later igneous rocks of local origin have undergone, being unproved?

From what has been advanced with reference to (a) the possible (solid-liquid) 'critical state' induced under great pressure on certain minerals, (b) the fluxing-action of traces of accidental mixtures, (c) the thermal energy developed by friction where the local mineral composition was such as to lead to crushing rather than incipient fusion,—it would be absurd to deny that slight changes of mineral composition (secondary paramorphism) may have occurred in some cases as the result of mere mechanical action *pari passu* with metataxis and metatropy. When however we realize the universality of the resultant operation of the combined factors of *time*† and ordinary chemical atomic change, are we not justified in saying that the *onus probandi* rests with those who would attribute the *whole observed result* (in any case) to pressure, and that the fallacy *post hoc ergo propter hoc* runs through much that has been asserted and written on the subject of 'dynamic or pressure metamorphism?'

Right truly has Kalkowsky remarked (*Lithologie*, p. 56): "Bisher ist der 'hohe Druck' in der Lithologie wie in der Geologie noch vielfach ein *deus ex machina*, der auftritt, wenn andere Erklärungsversuche nicht auszureichen scheinen;" which is as much as to say in plain English that pressure-metamorphism is a 'refuge for the destitute.'

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* In what has been urged in this work as to the chemical origin of the salinity of oceanic waters, and of the primitive quartzites and unfossiliferous limestones, I find I have quite independently and unwittingly trodden in the footsteps of Macfarlane ('Origin of the Eruptive and Primary Rocks,' pp. 71-73), and of Dr. Sterry Hunt, *(Canadian Naturalist*, vol. vii, p. 202, quoted by Macfarlane, *loc. cit.)*

† The writings of Suess and Heim, so far as they deal with the history of mountain systems as details of the larger subject, the history of continents, are of the highest degree of importance from this point of view.

CONCLUSIONS.

If now we review the facts and arguments contained in the foregoing dissertation, and take into account those given in the appendices, several important conclusions would appear to be suggested.

1. It is a vain and useless task to think of finding any *one principle of metamorphism*, since this is exhibited in Nature in various degrees and in various phases. In the higher and more complex phases of change all the four principles discussed in this work are exhibited as having come into play, not necessarily simultaneously, but in an order of succession determined by general laws of nature; in other words the laws of nature being (so far as we have any possibility of knowing) persistent, unchanged, and unchangeable, admit however of *an indefinite variation in the proportions* in which their operation is manifested in any particular field of action. The almost infinite variety of their collocations is not therefore so much a qualitative as a quantitative variation; and to these the variations in the phenomenal results must correspond. We have in fact all gradations of change, from a simple metataxic change in the cleavage of slate, or a simple metatropic change in the crystallization of a limestone into a marble after fusion under pressure, to the most complex changes observable in regions of contact-metamorphism.

2. When we come to consider what is commonly understood as 'regional metamorphism,' very large elements of doubt and uncertainty are introduced; and considerable non-provable assumptions have to be made in order to construct anything like a coherent theory. Some of the most important data upon which it has been attempted in some quarters to build up such a theory are found to admit of a different explanation from that which has been given to them by the advocates of the theory; and the relative values of these different explanations is not a matter of opinion, but a question of agreement or disagreement with known laws of nature.

3. The important revelations which the spectroscope (as a supplementary instrument to the telescope) has made in the last two or three decades, taken together with a firm grasp of the *principle of dissipation of energy** furnish data which cannot fail to throw light upon the history of the developement

* It is because this great principle is ignored, or at least but feebly grasped, that some geologists follow so blindly the rigid 'uniformitarianism' of which we hear so much. Until geologists of that school come to realize the vast importance of this one simple physical principle, we cannot admit that their creed is a 'rational' one.

of our Earth; the only assumptions being (1) that the same universal laws have operated in that developement as are seen in operation in other bodies which roll through space;* (2) that the *mass* of the earth including its atmosphere has remained the same from the beginning, its material having neither diminished nor (with the slight exception of added meteoric matter) increased. Deductions from the known principles of thermal chemistry and physics lead to the conclusion that in the history of its developement the Earth must have passed through a *pre-oceanic* stage, in which deposition of the more stable minerals occurred to form a non-consolidated crust in the presence at high temperature and pressure of that chemical body whose composition is represented by the formula H_2O; and in the minerals which for the most part constitute the archæan and pre-archæan rocks we can recognise just those which, from their known high degree of stability, we must regard as most likely to have been formed at such a stage of the Earth's developement. This leads to the further conclusion that the process by which the archæan gneisses and schists were formed (so far as their essential mineral-characters are concerned) was essentially 'diagenetic' rather than 'metamorphic.' If this be admitted, such phrases as "the highly-metamorphosed archæan gneisses and schists" must be relegated to an obsolete nomenclature of geologic science.

4. The archæan rocks themselves, wherever we have an opportunity of studying them in anything like their full developement, exhibit a general, though not uniform, progressive series of changes of mineral character, from the oldest mixtures for the most part of the most (thermally) stable mineral compounds, quartz and felspar (the granitoid fundamental rocks), through well-foliated gneiss (with its many varieties) into the schists and phyllites, in which the silicates themselves tell the tale of a gradual increase in the two main

* Pfaff *(Geol. als ex. Wiss.*, p. 31*)* thus summed up the evidence of this, fifteen years ago:

1. "No physical or chemical law, and in particular no known phenomenon can be brought forward, which would oppose the view of an original glowing (molten) condition of our Earth.

2. As a necessary consequence the oblate form of the Earth and the increase inwards of temperature, which we in fact observe, came about.

3. That we can recognise in the Sun and in other of the larger heavenly bodies the same glowing condition, makes it the more probable to us that the Earth was once in the same condition. The latter moreover, as one of the very small masses as compared with the Sun, must have cooled, so that its crust solidified, so much sooner than this can happen to such an enormous mass as the Sun."

factors which have determined their differentiation, *(a)* water,* *(b)* heavier (but less chemically-active) basic oxides, the high temperature of what we may call the 'Ur-gneiss stage' having been upon the whole too high for the formation of the silicates of the latter on anything like a general scale, and causing their deposition mainly as oxides, sulphides, and fluorides scattered as accessory minerals through the Ur-gneiss (in its many modifications) or precipitated more copiously in great intercalated metalliferous zones ('Fahlbänder') often miles in area. And the truth of this as a deduction is not affected, though the difficulty of reading the record of it is increased, by the vast disturbances (resulting in fracture, faulting, overthrusting, and overfolding) which have since interfered with the original order, and the enormous waste they have suffered in furnishing materials for sedimentary rocks, the proportion of SiO_2 (either free or in combination) in the one series and in the other being much greater than that of all their bases taken together.†

5. We thus come to regard the archæan series of rocks as representing upon the whole the primordial (first-formed) earth's crust, from which the siliceous materials of the sedimentary rocks have been for the most part derived: the

* This water was still for the most part highly superheated, in a condition, that is to say, such as that in which it operates in the high-temperature stage of contact-metamorphism in converting a clay-slate into a crystalline schist in immediate contact with granite, &c. Lawson's description of the crystalline schists in his Essay *(op. cit.)* may be referred to in illustration of this.

† Of all the chemical elements Carbon alone can be mentioned (after perhaps Hydrogen) as endowed with such marvellous and varied potentialities as Silicon. Whether we note their tetravalency as elements, their remarkable relative positions in the Periodic or Natural Arrangement of the Elements (as broached by Newlands, and since worked out to a greater developement by Mendeljeff, Reynolds, Carnelly, and others), or the part they severally play in the economy of nature, we can hardly escape recognising a sort of conjugate relationship between them. The chemical potentialities of Silicon being called out mainly at high temperatures, and those of Carbon at more moderate temperatures, they seem to stand, as it were, at the two opposite poles of matter, dividing the empire between them into what we commonly call the Organic and the Inorganic, but with very undefined boundary lines, along which dwell a series of restless and turbulent tribes, the individuals of which own no permanent allegiance to either, passing from the domain of each into the other in the most facile manner. Is it too much to hope that the day will come when the study of the chemical potentialities of the element Silicon shall become the focus from which new light shall irradiate the chaos of Mineral Chemistry, as the last decade or two have seen the confused accumulation of facts in the 'Chemistry of the Carbon Compounds' brought to a great degree into order and symmetry by the recognition of the simple principle, that, "in the chemical properties of Carbon alone lies the essential fact, which determines the peculiar properties the Carbon-Compounds possess, as compared with all others."? (Wislicenus, *Organ. Chem.*, § 1.)

fixation of CO_2 and oxides of some other non-metallic elements* as also possibly of the halogens, to form carbonates, sulphates, and haloid-salts, having continued on possibly into early palæozic times. The archæan stage of the earth's history is thus seen to fall into a place in a natural order of developement, and one more chapter is added to the history of the operation of that great Law of Evolution which is written upon all created things.

6. As the mists and clouds thus disperse our intellectual vision begins to descry a *boundary to geologic time*, and the physical geologist begins to feel that over this question he can join hands with the astronomer and the natural philosopher.

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* While much of the sulphur once existing in the elementary state in the primordial atmosphere may have combined directly with metallic vapours to form sulphides (the combustion of metals in sulphur-vapour being a fact well known in the laboratory), a large proportion of it no doubt underwent combustion into SO_2 with the further formation of free sulphuric acid; and this free acid no doubt played a part subordinate to that played by CO_2 in the decomposition of the earlier-formed silicates, the SiO_2 thus set free contributing to the production of the grauwacke quartzites. The insoluble sulphates of the alkaline earths thus formed were deposited, while the soluble sulphates of other bases were removed in solution. In some cases the sulphates may have been reduced to sulphides, which, as ores of the heavier metals (iron, zinc, copper, lead, &c.,) are of such common occurrence

Appendix i.

Notes of Laboratory-Work.

a. Sulphur.

Sulphur was obtained in the 'vitreous state' in two ways: (1) by pouring molten sulphur into cold water; (2) by just melting lumps of brimstone in an open evaporating-dish, allowing a crust to form in the first stage of cooling, perforating this and pouring out the portion still liquid. Some of the residual 'crystallites' (App. ii. Note F.) were examined microscopically. In some instances minute triangular thin plates of clear glass were found attached to the edges of the crystallites, giving them a very serrated appearance. These, when detached and examined microscopically, gave beautiful and interesting results.

At the ends of these crystallization sets in rapidly so as to point off the lath-shaped bands with devitrified wedge-shaped interstices. Gradually, but not uniformly, devitrification is seen setting in along the margins of contiguous bands of the glassy material, and here and there the crystallization cuts transversely into the glassy prisms, forming little nests of crystals, which, in a few minutes (in cases where they begin to form at opposite points of the prism) coalesce and completely intersect the glassy band.

As devitrification proceeds in its earlier stages a marked pleochroism is exhibited along the devitrified lines, while the bands retain in the interior their isotropic character as glass. In the course of half-an-hour with strong sun-light devitrification may be so far advanced as to render the sulphur opaque. In one very thin plate of sulphur-glass, upon which the full-powered sunbeam was allowed to fall directly as well as by reflection from the mirror of the microscope, I was able to watch the progress of the devitrification; and in that case the plate became opaque in five minutes. Sunlight is thus seen to aid in devitrification of sulphur as well as of phosphorus.

Latent heat of vitreous sulphur.

1.—Vitreous (plastic) sulphur obtained in the usual way by pouring sulphur at about 400°C into a deep jar of cold water.

2.—One portion of this on being hammered on a cold stone slab became in a minute or two so hot that it could not be comfortably held in the hand. The brown colour gave place to a pale yellow, partly stringy, mass very tough.

3.—The portion treated as in (2) was placed out of doors on a cold stone slab until it was cooled down so far below the temp. of the room as to show considerable *cold* (40° to 50° on the scale) when placed on the metallic face of the thermopile; yet after a few minutes exposure to the air of the room it gave a large reading for *heat* on the scale (100° to 200°) when again placed on the thermopile. For this purpose it was lightly pressed down on the face of the pile with a glass rod so as to get a large surface of contact. The mass hardened and stiffened rapidly.

4.—A piece of the same hammered mass was examined after between one and two hours under the microscope in reflected light (it had then become quite opaque) and was found quite crystallized, with the exception of a few small granules of brown amorphous sulphur.

5.—After about 18 hours the residue of the hammered mass had become quite hard, inflexible, and brittle, falling easily to a crystalline powder when broken.

6.—Experiments with unhammered soft brown portions of (1) after considerable exposure (1 to 2 hours) to the air of the room gave slight heat indications (about 10°) when laid on the pile; and 40° to 50° when lightly pressed upon it to secure a large surface of contact.

7.—Another portion of the fresh soft vitreous sulphur (1) was placed under a pressure of about 2 cwt. between two hard white porcelain tiles all night. In the morning it was about half devitrified, yellow and crystalline in those parts where the pressure was heaviest. When the flat plate of S thus procured was placed on the Th. pile (after some time of exposure to the air of the room) it gave a reading for heat of about 40°, and when gently pressed on the face of the Th. pile nearly 200°. Devitrification went on so rapidly that after 3 or 4 hours it gave only 10° when placed on the pile; and on examination under the microscope it was found to be almost completely crystalline. Next morning the plate was quite brittle.

8.—Portions of the vitreous sulphur (1) allowed to crystallize under ordinary physical conditions were not completely devitrified before the end of 3 to 4 days.

9.—A thin slice of the vitreous S. (1) when fresh was found to be quite isotropic in polarized light, but the crystalline structure developed itself rapidly from the edges inwards, and the next morning it was found quite opaque and devitrified.

The facts just described clearly point to the presence in the cases described of latent heat of vitrification, this heat escaping as devitrification progresses.

The specific heat of sulphur being ·118, we can calculate from the experimental results the *latent heat of vitreous sulphur* in the form of klinorhombic prisms as 2·27, since the rise of temperature which this form undergoes on sudden transformation into the ortho-rhombic form = 12°·1. (Wislicenus, *op. cit.*, § 236).

Again, the rise of temperature given (on the same authority) on page 37 of this work, enables us to calculate the *latent heat of the plastic form of sulphur*) thus:

·188 (specific heat of sulphur) × (110 − 93) = 3·196.

Whence also *latent heat of plasticity* = 3·196 − 2·27 = ·926.

b. Phosphorus.

1.—A portion of amorphous P. and a portion of vitreous P. were placed under a pressure of two cwt. between white porcelain tiles.

After about 18 hours the crystallization under pressure was very marked in both, particularly in the specimen of *red P.*; this was quite hemi-crystalline under the microscope, between two plates of glass, but showed no trace of pleochroism.

The compressed plate of *vitreous P.* exhibited a great variety of phenomena. A large part of its area was occupied by a vitreous 'basis' through which were scattered innumerable minute aggregates of crystalloids. There were also large patches of opaque amorphous P. A few faintly developed 'belonites' and 'globulites' were seen.

As moisture and O_2 gradually found their way in between the glass plates, crystalline bars and crosses were developed. These were brilliantly iridescent in polarized light, and especially beautiful between crossed nicols, when the ground around them was dark. The lines of colour on these rods indicated the prismatic form of the crystals, which must be regarded, I think, as H_3PO_4, free from hygroscopic moisture.

2.—Examined specimens of *(a)* red P., *(b)* vitreous P., both of which had been under pressure of nearly two cwt. for 6 days and nights continuously. Pressed plate removed and placed at once between glass plates which had been previously washed (1) with fuming HNO_3, (2) NH_4HO, (3) distilled water, then well dried.

In the red specimen *(a)* the same effect is seen as in the specimen before examined; but the crystalline portions transparent between crossed-nicols are larger and more definite in outline. Here and there bunches and prisms of well-formed crystals are seen.

In the vitreous specimen *(b)* a great variety of things were observed. Devitrification could be observed even macroscopically. Bunches and masses of red amorphous P appearing, some of which are soon seen to develope (in diffused light) crystalloids which a little later began to group themselves into crude crystallographic outlines.

Trichites, and microliths (straight) with obtuse ends were observed. Both classes of bodies with a very definite outline. Some of them quite *pellucid* (with vitreous interior) others semi-opaque (containing amorphous P.) Masses and patches of true crystalline P. of the regular system, and 'crystalloids' (Vogelsang) Zirkel *(Mic. Bes. der. Min. u. Gest.)* Globulites (with definite outline) some pellucid others semi-opaque. All these forms (except the crystals) undergo slow change with exposure to light.

After one or two hours a periphery of glassy HPO_3 was formed as a result of oxidation, and inside this (though not in every case close to it) a phenomenon of surpassing beauty was observed. In a transparent film of liquid matter a constant and rapid circulatory movement was seen which one could not help comparing with the circulatory movement of the protoplasm in the living cells of plants. This was at first very puzzling: but on further watching and reflecting upon the phenomenon, I saw my way to a probable explanation of it. A glassy mass was seen in the middle of the larger of these little areas, which was nearly opaque between crossed-nicols and produced no colour-effect upon sunlight transmitted through a nicol rotated below it. This I take to have been HPO_3 (glassy metaphosphoric acid); and the circulation in the liquid-film therefore but a manifestation of the molecule-forming energy which was being there displayed. The minute currents in the film were probably produced by the local generation of heat as molecules of H_3PO_4 were successively built up. Five or six hours later the vitreous mass (HPO_3) was much diminished and had parted into two small masses. Next morning the whole area had solidified and acquired the optical characters of crystalline H_3PO_4.

c. Silica.

1.—*A specimen of glassy silica* precipitated from Na_2SiO_3 as $SiO_2 + xH_2O$ several years before in my laboratory.

Portions found quite devitrified and opaque: too thick for examination by *transmitted* light. In strong reflected sunlight the whole surface was found to be crudely crystalline, by bringing successive elevations of it into the field of vision. Here and there, where the light pierced the thinner portions, the play of colours when the lower nicol is rotated showed the crystalline texture. Crystal-growth at edges very distinct; also in thin transparent patches of interior. With crossed nicols it allowed white light to pass freely.

2.—*The fine impalpable powder* precipitated several years ago in the same way as before was examined in transmitted light, also the fine powder obtained by the desiccation in the air of the laboratory of the hydrated filtered-off gelatinous precipitate obtained by passing silicon tetrafluoride through water, by the well-known reaction—

$$3\,SiF_4 + 4\,H_2O = 2\,H_2SiF_6 + Si(OH)_4.$$

Definite *crystalline form* detected only in a few particles. The particles generally appeared to be sub-crystalline masses of partly devitrified glass, producing a certain play of colours when rotated in polarized light, and giving free passage to white-light when viewed between crossed nicols. In a few of these fine particles pellucid globulites were well seen with a $\frac{1}{4}$-inch

objective. Generally the particles are distinctly angular, some of them more elongated with pyramidal ends, the angle of the pyramid being that of rock-crystal, and in some instances minute pyramidal crystals were seen as a growth on the edges of the larger masses. In one case the minute crystallites were seen (¼-inch Ob.) grouping themselves into a crude crystalloid of the normal shape of rock-crystal pyramidal at each end.

3.—In a specimen of glassy silica only two or three months old beautiful moss-like growths of *crystalloids* were seen in the translucent margins of the thin edges of a partially devitrified mass. The colour-play when a rotated beam of polarized light was passed through them, and their extinction between crossed nicols established their crystalloid character. (Zirkel).

4.—Examined under the Mic. specimens of hydrated SiO_2 powder which had been precipitated about three months.

The *minute powder* was made up of angular particles of devitrified SiO_2 as in the older specimens examined before. A few *larger plates* were obtained from this powder, strongly *iridescent*. Under the M (+Nic) a splendid *spherulitic structure* was observed; very marked and very beautiful: some of the discs well rounded, others with a wavy periphery. The plates were too thick to give well-defined outlines of these spherulites, with powers higher than 1-inch obj; but the play of colours when a Nic. was rotated below in bright sunlight, seemed to indicate some incipient crystallization.

It was impossible to avoid being struck with a certain resemblance of this spherulitic structure to that observed in some volcanic glasses.

5.—A portion of clear solid glass of hydrated silica, which had been precipitated about three *months* (A), and a portion of *more devitrified glass* which had been precipitated about three *years* (B) were gently dried and weighed, then calcined in B-pipe furnace for over half an hour.

Results:

A was melted down to almost a clear glass at the bottom of the crucible with perfect extinction between × nicols and was found to have lost 50·3 per cent. of water by weight.

B remained as a porous pumiceous mass and was found to have lost 23·9 per cent. by weight of water.

Slow devitrification is therefore attended with *loss of water* in this case.

d. Solvent action of the Humus-acids.

Little is to be learned from the scientific literature of this country of these acids, which consist of a series of carbon-compounds of no great stability. As acetylene is a hydro-carbon product of incomplete combustion of lighter hydro-carbons, and therefore in one sense a reduction-product from them, by the partial combustion of their hydrogen into steam; so in the process of slow oxidation of organic matter these bodies result from incomplete oxidation of the higher carbon compounds which are formed in the earlier stages of decomposition in water of woody fibre and other vegetable tissues. Ultimately they may be oxidized into CO_2 and H_2O, which are the ultimate products of combustion of the same non-nitrogenous substances. The humus-acids are found to be capable of replacing CO_2, and appear to be intermediate in energy as acid constituents of salts between acetic and carbonic acids. Much experience of waters containing them including analysis of many samples has shown their potent solvent action on iron, lead, and zinc among the metals. All this has been more fully treated of in papers already published.* Here we are only concerned with the part they play as *solvents of minerals*.

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* See paper by the author in *Proc. Inst. of Civil Engineers*, vol. lxxxv. (1885-6).

Some time ago a favourable opportunity offered itself to the writer for procuring a considerable quantity of these colloid extracts, as the result of precipitation in an open storage reservoir, which received the overflow from a spring issuing from the sands at the base of the Upper Bagshot. A year or so previously a drain had been laid for some distance to this outflow a few feet underground. The drain was constructed of open-jointed pipes; and to prevent the sand getting into the pipes a great quantity of heather-litter was rammed in above them before the trench was filled up. The decay of this underground charged the water with organic matter; and, as this was only incompletely oxidized in the storage-reservoir, the cemented bottom of the tank became covered over in about six months with a slimy colloid mass, containing some 50 per cent. of crenic acid freely soluble in alkalies. It occurred to me that it would be interesting to ascertain experimentally the effect of this upon some of the minerals which form the more common constituents of many rocks. For this purpose two or three fragments of each mineral were taken (all dust being carefully eschewed, in order to avoid errors in weighing due to any accidental loss of it), well dried in an air-bath at 120°C, then placed in separate flasks with 250cc. of distilled water in each, and approximately equal quantities of the fresh colloid material taken from the reservoir. The flasks were tightly corked, and allowed to stand at the temperature of the laboratory as long as the water continued to give off a perceptible odour, resembling that of ordinary pond-silt. This they continued to do on being occasionally opened for about three months. After the odour ceased, the organic acid being presumably destroyed by slow oxidation, the minerals were taken out, dried again severally at 120°C, and weighed. The results are given in the following table:

Mineral or Rock.	Original Weight dried at 120°C.	Weight after treatment for 8 months.	Loss.	Percentage of Loss.
	gms.	gms.	gms.	
Limestone (Hallstatt) . .	4·280	4·265	·015	·350
Rusty iron 'pan,' part of a conglomerated-gravel . .	3·160	3·090	·070	2·215
Chalk Marl	3·195	3·125	·07	2·035
Muschelkalk	3·070	3·060	·010	·326
Hornblende	1·772	1.780	—	—
Augite	·770	·775	—	—
Orthoclase	·870	·857	·013	1·530
Sanidin (from the Trachyte of Drachenfels) . . .	3·125	3·110	·015	·480
Bath Oolite	3·029	2·970	·059	1·976
Oligoclase	1·055	1·050	·005	·476
Obsidian.	·620	·620	—	—
Mica (Muscovite) . . .	·580	·580	—	—

From these results it would appear that of the minerals experimented upon ferric oxide yielded the most readily to the solvent; next in order follow the more earthy varieties of limestone; then the felspars and more compact limestones; while no solution at all of horneblende, augite, mica or obsidian appears to have taken place. The very slight *increase* of weight in hornblende and augite may be attributed probably to the oxidation in the process of drying of the iron contained in them; and this is borne out by the fact that the augite crystal (from Bohemia), which was quite clean and unweathered before the experiment, was found afterwards to present a somewhat rusty appearance. In order to obviate the error which would otherwise have been introduced by the oxidation of protoxide into peroxide of iron in the drying process in the case of the fragment of iron 'pan,' this substance was dried under a weighed portion of fresh-calcined animal charcoal, by which means the access of atmospheric oxygen to it was prevented.

The solvent action of organic (humus) acids on the oxides of iron pointed out here, seems to throw light upon the occurrence of Carboniferous 'white trap,' as described by Allport (Q.J.G.S., vol. xxx, pp. 538, 550, 556). The vegetable-matter locked up in the coal-measures would furnish these and allied acids. In addition to the researches of Berzelius, Boutigny, Mulder, Julien, and others, reference may be made to the researches of Williamson (quoted by Rammelsberg, *Mineralchemie*, p. 726) on the so-called Bogbutter from the peat of Ireland, as establishing the veritable acid character of these bodies.

Allport *(op. cit.)* states also that "wherever this green rock (at Deepmore) occurs, it invariably becomes lighter in colour as it approaches the coal or shales, and near the junction is nearly white. In this state the constituents are completely decomposed, but may still be recognised under the microscope, the original forms being preserved. The iron appears to have been removed, and the mineral is represented by pale grey pseudomorphs. Near the line of junction the trap is always quite as much altered as the shales and coal, and the alteration consists mainly in the decomposition of the ferruginous silicates." (pp. 549,50.)

e. Devitrification of Flints. (cf. p. 46).

With reference to the devitrification of flints the following note may be of some interest. Two specimens, in which marked changes had taken place by prolonged exposure, were selected for microscopic examination.

Flint A. is so completely devitrified as to have lost entirely all the character of flint through an irregular layer about a third of an inch in average thickness. It has become porous even macroscopically, and many of the pores are filled with amorphous Fe_2O_3.

Flinty band: (macroscopically compact) micro-crystalline ground-mass between × nicols; little specks of chalcedonic material scattered through it; some amorphous earthy-looking dust (probably, from the rusty colour of the slide, Fe_2O_3). On its outer surface this flinty band has assumed a porcellanic character such as I have described elsewhere * as occurring on the exterior of sarsens and fragments of millstone-grit, as the result of exposure.

Cherty band; (macroscopically stony and somewhat porcellanic, with quite conspicuous perforations); ground-mass much more completely crystalline; perforations often lined with the deposited ferric oxide, which seems to have been concentrated on their walls. The most striking feature however here is the occurrence of secondary quartz in flint, highly luminous between × nicols. It forms a sort of second wall behind the ferric oxide which generally lines, and in some cases fills up, the perforations. There can scarcely be any doubt I think from the relation which this 'mosaic-like' quartz is seen to bear to the

* Pres. Geol. Assoc. Vol. viii, pp. 159-160.

ferric oxide, that the two are concomitant decomposition-products from solutions of double azo-ferrous salts of silica and an organic acid. Special interest attaches to the production of a 'mosaic-like' structure in quartz in the 'wet way (*i.e.* through the intervention of water), so that such quartz has no necessary connection with tridymite. The perforations are scattered with a tolerable regularity through the mass, and are so approximately equi-distant as to suggest the 'pores' or 'oscula' of a sponge. Whether this is the true explanation or not of this interesting structure, we may safely assert that the action of the solvent at a later period on this part of the flint has been much greater than its action on the other part, in which the flinty texture has been much less obliterated; and we should not probably be far wrong in attributing this difference to a difference of molecular structure in the deposited silica of the original flint, owing perhaps to the more direct action of decomposing sarcodic material in the one part than in the other.*

Flint. B. has a similar porcellanic character on the exterior to that described already as occurring on the outer surface of A, but not quite so compact. This flint is upon the whole less altered than A, but there appear to be in places irregular masses of purer quartz than in the rest of the slide. The ferric oxide has undergone concentration in a narrow zone just beneath the more porcellanic outer layer. In the latter the ground-mass is distinctly micro-crystalline and there is much opaque amorphous material which does not seem to be wholly made up of ferric oxide. At two or three points are seen structures with a striking resemblance to 'spherulites.'

In order to get some idea of the extent of the part which hydato-devitrification plays in the degradation of flints, I have compared the loss of weight on calcining, which three selected and fairly typical specimens were found to suffer by heating to a glowing red heat for half an hour.

The following were the results:

		LOSS.
No. 1.	Specimen of Sarsen-stone from Netley Heath (rather 'flinty')	·852 per cent.†
No. 2.	Specimen of rusty flint which had undergone a moderate amount of weathering.	1·674 „ „
No. 3.	Specimen of a bleached and chertified portion of flint.	3·125 „ „

The percentages are, as will be seen approximately in the ratio 1 : 2 : 4. Hydato-devitrification seems therefore well exemplified in these changes. On the other hand, a specimen of native black flint lost just 4% on being treated in the same way. Loss by combustion of carbon, which amounts to about ·06-·07% has been neglected.

At a recent meeting of the Geological Society (March 28, 1888) some remarkable examples of corroded agates were shown by Prof. V. Ball in illustration of a paper by him "On some Eroded Agate Pebbles from the Soudan." My observations on the action of humus-acids on flints in the Bagshot country led me to suggest that the humus-acids furnished by the desert-scrub appeared to me from a consideration of all the facts of the case as the most likely solvent; for solvent action of some kind must have acted to produce such features as the agate-specimens exhibited. I also stated that Rammelsberg had pointed out that many minerals after being fused into a glass are more readily acted on by solvents than in the crystalline state, which, from considerations adduced, I regarded as due to difference of molecular structure. Reference has already been given to this statement of Rammelsberg's on page 45 of this work. I also stated on that occasion that while the replacement at fusion-temperature of CO_2 in alkaline carbonates by

* The secondary quartz observed in this flint seems to me undistinguishable from that observed in one of the altered "ash-beds" of Snowdon (see p. 59), and in some of Allport's specimens of devitrified pitchstones in the South Kensington Collection. (B. Mus., Nat. His.)

† On the cementing (secondary) silica probably.

SiO_2 was a matter of common laboratory-experience, it was not so easy to say what the action might be in the 'wet way'; though it was impossible to deny that SiO_2 in some of its allotropic forms might replace CO_2, and thus form alkaline silicates which would be removed in solution. I have since investigated this matter in my laboratory, and the result is an affirmative one.

1. A flint-nodule from the Chalk was split in two, and was found to be entirely bleached and to have acquired the cherty character referred to above to the depth of a quarter of an inch from the surface. Portions were chipped off from this outer zone, and some of the cleanest selected. These were pulverized in a mortar and boiled in a saturated solution of Na_2CO_3 for some time (one to two hours). On filtering and nearly neutralizing the filtrate with HCl, a precipitate of gelatinous silica was formed in one part of the filtrate which was allowed to stand for a day or two, and more quickly in another portion which was gently evaporated down on a water-bath at about 60°C.

2. A fragment of fresh unaltered native black flint from the Chalk was subjected to the same treatment: and the precipitation of gelatinous silica was even more marked in this case than in the first.

There can be no reasonable doubt after these results that at a boiling-temperature silica in some of its modifications is capable of replacing CO_2 to a slight extent in concentrated solutions of alkaline carbonates and thus passing into solution as an alkaline silicate. But we can scarcely infer such action from these data in the case of the Soudan agates mentioned; and we are thrown back upon the action of decomposing organic matter (perhaps when the drainage of the locality was different) as the most probable origin of the solvent which corroded these agates.*

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* In revising this work in its present form it may be as well to refer those gentlemen, who take upon themselves to deny (in the name of "English Chemists") the existence of these acids, to the recent researches of C. G. Eggertz. (*Biedermann's Central-Blatt für Agri. Chem.*, vol. xviii, part 2.) Such tricks of debate are worthy of the 'professional expert.'

Appendix ii.

Supplementary and Explanatory Notes.

Note A.

Reduction and Dissociation in Volcanic Action.

Davy's well known suggestion that crateral explosions might result from the escape of large bodies of free hydrogen, as a result of the reduction of water (or steam) by the action of the metals of the alkalies and the alkaline earths, is scarcely admissible, as it stands; since at our highest furnace-temperatures the oxides of these metals are not found to be dissociated, and in the case of volcanic phenomena we have to add another factor, in the retardation of dissociation due to the pressure that must exist in the volcanic canal. Yet the *idea* which underlies Davy's suggestion contains more in it than appears at first sight. The facts (1) that all the more basic lavas contain a considerable quantity of the metals *iron* and *manganese* (not to mention any others); (2) that their existence either as basic constituents of volcanic minerals and as peroxides in the lavas as we know them, and which we have no opportunity of examining in the volcanic canal, or in any stage before their propulsion into the air and consequent further oxidation by atmospheric oxygen, is no proof of their non-existence as lower (basic) oxides or even as free metals (and therefore as reducing agents) within the canal; these facts taken together seem to point to the *idea* of Davy as worth more consideration than it has generally received from geologists.

I have searched in vain for any recognition of the idea of Davy as here elaborated both in Geikie's and Green's text-books, in neither of which does it appear to be even touched upon; and in a recent paper before the Royal Society, Prestwich refers to Davy's suggestion only to dismiss it with a passing remark or two. The late Prof. Phillips in his interesting and suggestive work *(Vesuvius)* in criticising Dr. Daubeny's views (p. 320) comes so near the idea, that there can be little doubt that a little more knowledge of thermal chemistry would have led him to see a reasonable cause for the occurrence of great masses of "unmistakable free hydrogen in a blaze above the mountain." The occurrence of such free hydrogen, would of necessity lead to explosive combustions as it mixed with free atmospheric oxygen; and herein the brilliant and versatile mind of Phillips would probably have seen a more efficient explanation of the production of great clouds of steam, like piled-up bales of wool, to which he so frequently adverts in the work named, than in the hypothesis of the 'sphæroidal state' referred to in the text. The fact of flame actually occurring in some cases, though not in all, is too well attested (see Phillips, *loc. cit.* pp. 153,4) for us to be satisfied with a mere dismissal of the subject as an 'illusion' Huxley, *Physiography*, pp. 190-91. The same writer *(loc. cit.)* seems to confuse the products of combustion with the combustible gases, which we have no means of directly examining but spectroscopically. With this method we ought to be able to settle the existence or not in definite instances, of glowing hydrogen at the volcano's mouth; and until its existence has been thus proved, it is perhaps not very profitable to speculate as to its origin.*

There is a further point raised. We might ask the question whether mere heat might not *dissociate* the steam contained in the glowing lava, as soon as it was released from the pressure of the canal. Now we know (as I have pointed out elsewhere, *Chemical News*, vol. liv. (1886, No. 1402) that the

* From a conversation with Dr. Johnston-Lavis during last year I gathered that his observations of the eruption-phenomena of Vesuvius during recent years do not lead him to assign any important part to the combustion of hydrogen in the explosive phenomena observed.

temperature of initial dissociation of steam lies somewhere between the melting-points of silver and platinum; but for the complete dissociation of steam we require a temperature far above the melting-point of platinum, and above the temperature even of the oxyhydrogen flame. How far such conditions are realisable in a glowing lava issuing from a volcanic canal can only be determined by an extensive series of observations; but that an approximation to such conditions is not improbable, may be seen from the fact (Geikie, Text-book, p. 227) that, after a lava-stream has so far cooled as to allow the observer to approach it, its temperature is found in some cases to be, even near the surface of the mass, above the melting-point of silver. If dissociation occurred in this way, we should—it is hardly necessary to point out—refer these paroxysmal explosions to the presence of oxyhydrogen gas, which, as I have often shown in a lecture-experiment, takes place in a glass-tube at a feeble red heat of the glass.

Further systematic observation of volcanic eruptive phenomena may show that there is some truth in both the explanations here suggested. To settle which of these two hypotheses furnishes the more efficient explanation of these crateral explosions it will be necessary to connect them with the general nature of the ejectamenta. Should it turn out that the phenomenon is associated only or principally with the *basic* ejectamenta, we shall have to give the palm to the former—the *reduction-hypothesis*; on the other hand, should it be found to be associated equally with basic and acid ejectamenta, the latter—the *dissociation-hypothesis*—would have to be recognised as pointing to the more efficient cause.

In this *discursus* we are concerned only with the explosions and consequent piling-up of a pillar of dense clouds; the tension of confined steam, as the main factor in bringing about the explosive phenomena, to which the clastic products of vulcanicity are due, is another question altogether. A comparison of my own observations on the abundance of tuff interbedded with such acid lava-flows as those of the Dyas-period in the Rittner and Grödner regions of the Alps and the mixed nature of the highly basic ejecta of the Vesuvian and Phlegræan regions seems rather to suggest that this is common to both kinds.

With regard to the possible occurrence of iron in the elementary state a reference may be made to the observations of Nördenskjold on the west coast of Greenland, in 1870. If iron occurred in anything like such quantities in a molten lava in the canal of a volcano, it is clear that we should have an agent present capable of reducing steam to free hydrogen on a grand scale; and laboratory experiment teaches us that the oxide of iron formed in the reduction of steam at red heat is Fe_3O_4, which as magnetite is a common constituent of basic igneous rocks.

It is difficult to conceive what the late Prof. Phillips could have meant by the "cooling of water-bubbles below the spheroidal state to some red heat." Could he have been thinking all the time of the "critical state" of water? (See *Vesuvius*, p. 265).

Note B.

Vitality and crystal-building. When Prof. Judd's brilliant Address appeared, in 1887, I took an opportunity of pointing out to him the visionary nature of his speculation. I said—"when the chemist shall have succeeded in producing synthetically one particle of *living* protoplasm, we shall be able to make a real departure in the direction indicated in the Address." The importance of it may at once be seen by the emphasis laid upon it later in the year both by Prof. Sir H. E. Roscoe, in his Presidential Address to the British Association at Manchester, and by Dr. Schunck in his Address to Section B, at the same meeting.

Again, the *growth* of minerals by accretion was fitly contrasted by Dr. Woodward (in his Address to Section C), with the growth of living organisms by assimilation and intussusception.

Or, once again, the trite phrase, "ohne Phosphor kein Gedanke," no doubt expresses a truth, but *not the whole truth*. That phosphorus is an essential constituent of the hard grey matter of the brain-centres is one fact; and that mental strain is shown by analysis of the excretory products to involve a more than average expenditure of this, is another fact; yet these facts *tell us absolutely nothing as to the state* (high or low) *of combination* in which the element itself exists in the living active organ of thought. All the phenomena seem adverse to the hypothesis here criticised; as those phases of activity which we commonly call "vital" seem to be connected with matter in a transitional state (waste and replenishment of the organ being the necessary condition of vitality), in, so to say, a low degree of *molecular* structure giving the freer play to *atomic* forces.

Note C.

The Hypothesis of a metallic kernel.

Osmium the heaviest of all known bodies, (sp. gr. 22·48) and Ruthenium with a sp. gr. of 12·26 (between those of lead and mercury) are even more difficult to fuse than platinum, osmium having never yet been fused even in the oxyhydrogen flame. Iridium again has a higher Sp. gr. than platinum. Both osmium and iridium occur in platinum ores (in the undissolved residues of which they were first discovered) as alloys with that metal. From considerations such as those advanced in this work, and from a comparison of the sp. gravity of the globe with the specific gravities of siliceous rocks, it would appear likely that these exceedingly dense and refractory metals and alloys were the earliest of all *known* forms of matter, to condense and form the solid core or nucleus of the globe; and it may be that some of our so-called "rare" metals are only rare at or near the earth's surface. It is even possible that other and even denser forms of matter entirely unknown to us may exist. I have elsewhere discussed the two factors, conduction of heat and pressure, in relation to the possible existence of materials in a state of fluidity at intermediate depths. See British Association Report, Birmingham Meeting (1886), pp. 657,8.

Note D.

On wet and dry reactions.

The old adage "corpora non agunt nisi soluta" is perfectly well known to the modern chemist to be a formula for a generalization based on insufficient data, which is only a partial expression of the truth that *molecular contact is a necessary antecedent of chemical action*. This is the simple explanation of such bodies as, for example, dry sulphur and copper (quoted by Mr. Harker in his essay at p. 847) being made to combine under great pressure by M. Spring.

The truth, in its more general range, may be illustrated by a very simple experiment. Mercuric chloride ($HgCl_2$) reacts upon potassic iodide (KI), as is well known. If 4 gms. of the former and 5 gms. of the latter be dried separately at 100°C in the form of coarse powder, and roughly mixed by shaking round in a dry flask, a certain pinkish hue, due to the formation of mercuric iodide [$HgCl_2 + 2KI = HgI_2 + 2KCl$] is at once perceptible in the mixture. This deepens to a bright pink red when further molar division of the mixture, effected by rubbing it in a mortar, increases the number of *points of contact* of the solid particles (so marked is the colour that it may be exhibited to a large class in a lecture). But even this is feeble as compared with the instantaneous production of the intense lovely hue of HgI_2 obtained by mixing together aqueous solutions of the two salts previously prepared separately in the ratio indicated above, which is an approximation to that of their 'equivalent proportions.'

NOTE E.

Sedgwick's Hypothesis as to 'Waves of Heat.'

As an instance of the unscientific play of the imagination we may note the *imaginary* basis of a theory proposed by even the great Sedgwick, when he conceived that "both cleavage and foliation are due to the parallel transmission of planes or waves of heat, awakening the molecular forces and determining their direction"—a view endorsed (as it appears) by Prof. H. D. Rogers. The unreality of the basis on which it rests is seen at once by bringing it to the test of a few simple physical principles, based on unquestionable experimental data.

We know of three ways in which heat is transmitted:

1. Radiation,
2. Convection (in fluids),
3. Conduction.

The first is clearly out of the question; the second is equally inapplicable to a solid mass; the third is nothing more than a tendency towards equilibrium in the heat-energy contained in a system, by virtue of which when masses of matter are in contact at different temperatures, there is a constant transfer of heat from the hotter to the colder masses until equalization of temperature throughout the system is attained. But the propagation of a series of 'waves of heat,' would require also *the converse of this*; which is about as possible as that water should flow up-hill under the mere influence of gravitation. The apparent analogy of the propagation of sound-waves in such an elastic medium as air (coupled with the obsolete caloric-hypothesis) where the alternate condensation and expansion of a *material body* form the counterparts of each other in every undulation, probably misled Sedgwick.

NOTE F.

The terms 'vitreous' and 'amorphous.'

It will be seen that in this work the term 'amorphous' is used in a more limited sense than is common among mineralogists, with whom it is customary to include those forms of matter which are here distinguished as 'glassy' or 'vitreous.' (See Rammelsberg, *Handbach der Mineralchemie*, pp. 38—40). That authority defines (p. 38, *loc. cit.*) amorphism as follows: 'Die Masse eines festen Körpers wird amorph genannt, im Gegensatz zu krystallisirt, wenn die Kennzeichen des krystallisirten Zustandes ihr fehlen." The facts considered in the thesis in connexion with devitrification have rendered it necessary to adopt the nomenclature of chemistry rather than of mineralogy; since its aim is rather to get at the principles—the natural operations—which determine those phenomena, the description of which is the proper function of the pure mineralogist At the same time the difficulty of drawing a sharp line of distinction between non-crystalline bodies is not unperceived, since these graduate from the condition of a true *glass* through the various phases expressed by the terms 'opalescent,' 'gelatinous,' 'flocculent,' to that which is implied by the term 'amorphous,' as it is used in chemistry. The following examples of variations in spec. gravity with allotropic forms given by Rammelsberg (*loc. cit* p. 39) may be compared with the cases cited in this work:

	Amorphous	Crystalline
Sulphur	1·92	1·96—2·07
Selenium	4·28	4·75
Phosphorus	2·18	1·82
Silica	2·20	2·3 and 2·6
Antimony sulphide	4·28	4·60
Orthoclase	2·34	2·56

On this table it is to be noted:

(1) The lighter form of 'crystalline' sulphur given as having a specific gravity of 1·96 is the *sulphur-glass* of the prismatic *crystallites*, which have

generally (I believe always) been hitherto described by chemists as 'crystals;' upon the strength of which sulphur has been spoken of (as it would appear erroneously) as a 'dimorphous body.'

(2) The same confusion appears in the case of phosphorus, and introduces what appears at first sight an anomaly, which, were it anything more than an *apparent* exception, would vitiate the generalization enunciated in this work; but on the other hand causes no difficulty, when it is seen that it is in reality the *vitreous* form of phosphorus which has the density of 1·82.

(3) The 'amorphous' orthoclase is really the glass.

The facts are quite sufficient to justify the distinction which has been drawn in the text of this work between vitreous and amorphous bodies in the chemical sense.

From recent observations I have been led to recognise vitreosity as a phenomenon occasionally exhibited by water under conditions favourable to rapid loss of heat at 0°C. (See *Nature*, vol. xxxvii, p. 104).

The facts cited certainly lend support to the view advocated in this work as to latent heat of vitrification; and the fact of the greater brittleness in frosty weather of ordinary window-glass—which is a matter of common observation—may perhaps be explained on the same principle. Further investigation may possibly also lead to our recognition of vitrification as the real explanation of the silvery blue tint which characterizes 'glacier-ice.'

Note G.

On Fritting.

Since the suggestion as to the possible formation of Wollastonite by direct replacement of CO_2 by SiO_2 in the dry way was written, I have had the privilege of seeing in Dr. Percy's collection a fine specimen of Wollastonite, made some years ago in his laboratory, by heating finely powdered calcite and fine clean quartz sand together in equivalent proportions. On turning to p. 46 of the new edition of the volume on *Fuel, &c.*, by that distinguished metallurgist, we find the rule laid down, that 'Silica combines readily with a metallic oxide, when an intimate mixture of the two is heated to the right degree, *provided the oxide be not reducible, per se*, at or below the temperature required for combination.' And he points out that *fusion* is not necessarily required for combination (*e.g.* in the case of silica and lime), and in metallurgical operations in the process known as 'fritting,' is to be avoided with an easily fusible oxide of a heavy metal (*e.g.* protoxide of lead).

The occurrence of Wollastonite as a product of contact-metamorphism has been recorded by Prof. Heddle in the marbles of the north-western portion of the Scottish Highlands. (See Q.J.G.S., vol. xliv, p. 411).

The fact that 'slags' are composed of silicate of lime and other silicates formed in the *dry heat of the furnace*, is most important in its petrological bearing; and must always be borne in mind as a caution on instituting any comparison between them and the silicates found in the crystalline rocks, —the *schists* on the one hand, and the *eruptive rocks* on the other. This consideration too tends to confirm the theory advocated in this work, that the original minerals of the schists have crystallized in the presence of superheated water, though not 'precipitated' in aqueous basins.

Note H.

Orographic Structure of the Alps.

The importance of looking at the structure of a comparatively young mountain-system is seen from the fact that most of the mountains of these islands are but the worn-down stumps of ancient mountain-systems. Non-recognition of this fact has it appears, led to some erroneous theorizing among the untravelled members of the British fraternity of geologists: they have mistaken the abnormal for the normal relation of things, and have been in consequence a little too ready to arrive at advanced theories as to

'regional metamorphism.' The recent publication of Prof. Heim's great work *Mechanismus der Hochgebirgsbildung* has opened their eyes to some leading principles which have been for some years more or less patent to the leading geologists on the continent (Switzerland, Austria, Germany). I shall sketch here briefly a few of my own observations made during several traverses of the main or central chain of the Alps in past years (chiefly from 1877 to 1883); premising that the general orography of the Alpine system —a central chain of crystalline rocks with flanking chains of sedimentary rocks, (mainly of Secondary and Tertiary ages)—is known to everyone who takes an interest in this subject.

My first traverse was made from Flüelen by Altdorf, Amsteg, and the Kreuzli Pass to Sedrun and Dissentis in the Vorderrheinthal. This pass is somewhat rough and unfrequented, but has the advantage (to the geological observer) of being free from glaciers. One could not fail to be struck, as one penetrated the mountains from the north, with the distinct succession of slates, schists and gneiss into the alpine granite which seemed to form the backbone of the range; the same succession (in part) being seen in reverse order as one descended to the V. Rheinthal, and in a walk down the Reussthal another time from Andermatt to Amsteg. The valley of the Vorder Rhein is apparently formed by a gigantic flexure in the main chain, which has left the oldest rocks covered up by a series of younger schists of undetermined age, including the Bündner* and Casanna Schiefer (the "schistes gris" and "schistes vertes" of the Swiss geologists), and some later formations, including the Verrucano. Leaving the main valley at Ilanz, and striking up the side valley to the south, one passes through these younger phyllites and schists, all the way up the valley to Vals-am-Platz. Near Furth I found talc-schist, and higher up the valley the green schists (schisteverte) and marble (calcaire indéterminé) interbedded with the grey schists (schists gris). I have twice traversed this valley, and have thus repeated the observations. The second time one had rather exceptional advantages from the fact that a new road was in process of construction, and the blasting-works had laid bare large exposures of fresh rock-surface. The foliated structure of these rocks at a very high angle has facilitated the erosion by the mountain stream of most magnificent gorges, quite comparable with that of the Via Mala for depth and narrowness. On my first traverse the Pass of the Valserberg was covered with snow; but on the second occasion it was free from it; and I observed what appeared to be the green schists forced in contorted masses into the older schists.† On referring to Studer and Escher's map I find the green schists and marble shown as cutting through this range into the Hinterrheinthal, between the older mica-schists and the grey-schists. These last are met with down the Valley as far as Splügen. In the road cuttings between that place and Andeer the mica-schists and gneiss are well exposed, and have a decidedly older look than the schists of the Valserrhein. Two different routes were taken on the two several traverses from the last-named place to Bivio-Stalla, at the foot of the Julier Pass.

On the first occasion the wild steep rough valley of the Averser Rhein was followed up to Cresta (the loftiest Swiss village according to Bädeker). The lower part of this valley has a wildness and ruggedness almost peculiar to it in my experience; and on referring to the map, this seems to be accounted for by the fact that it is cut through the oldest schists and gneiss which

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* Local geographical name (Von Hauer) from the territory of the mediæval 'Bund.'

† Field observations on these green schists of this region of the Alps certainly suggested even at that time (1878) an igneous and intrusive origin for them. (cf. Kalkowsky, *op. cit.*, p. 217). They are probably the Valrheinit' of Rolle.

are much more indurated than those of the younger series of schists developed higher up the valley. Mining for metallic ores is carried on in the older series. Crossing over the Stallaberg, dykes of green serpentine with a pseudo-fibrous cleavage are seen cutting through the mountain and well exposed by the side of the mountain-path which crosses them.

On the second occasion the route was from Andeer by the Via Mala to Thusis; thence by the Schyn Pass, Tiefenkasten, and the Oberhalbsteinthal to Bivio. The deep-cut steep gorges of the Schyn reminded one of those of the Valser Rhein, and this character the two valleys seem to possess in common from the fact that they are both cut into the younger schists, as in fact is the gorge of the Via Mala, where the rocks are of a more calcareous character; an instance of these grey schists becoming in places massively kalkhaltig. (Gümbel). Green and red serpentine is so abundant in the Oberhalbsteinthal as to be used commonly for road material; and about Mühlen this mineral appears to permeate the 'green schists.' I obtained opposite the hotel at this place a specimen of a highly-contorted portion of these schists, with marked foliation. Ascending the Julier we have (1) Casanna Schist, (2) Gneiss, (3) Granite (intrusive); and portions of the schists are found above the Pass; while lower down, on the south side, a quartzite (probably grauwacke) is quarried by the road-side. These appear from their position to be portions of once-larger masses infolded in the granite and gneiss in the process of mountain-building. I have studied the gneiss and schist of the Upper Engadine from the Maloja Pass to Samaden, and have been impressed with the strong evidence in them of some sort of bedding. The serpentine too of the Oberhalbstein was observed cutting through into the valley above Silvaplana. The granite of the Roseggthal, the gneiss about Samaden and St. Moritz, the granite and gneiss of the Bernina Strasse and the schists of the Alp Grün have all been studied 'in the field.'

On one occasion I returned from the Engadine by the Suvrettathal and the Val Bevers, then over the Weissenstein granite massif, dropping down to the Albula Strasse near the top of the pass. Walking down to Bergün, good road-sections were seen in the Liassic slates, the cleavage of which was so pronounced, that the eye could follow its strike through the mountains to the north west. On petrological grounds alone, and quite independently of their organic remains, I should say there was no difficulty in distinguishing these slates from the phyllites of the older (archæan) series.

From Bergün Davos-am-Platz was reached by the Sertig Pass and Frauenkirch, the older schists being seen in the lower part of the Sertigthal. From Davos Landquart was reached by the road through the Prättigau, the valley traversing the younger or grey schists and phyllites. On another occasion the same series of schists and phyllites was studied in a journey from Tiefenkasten by Lenz and Churwalden to Chur.

Impressions strongly forced in upon one's mind in these travels and observations were such as the following:

(1) The fact of a general order of succession prevailing in the oldest and most highly 'metamorphosed' strata of the Alps;

(2) The existence of more than one series of schists, those of some districts having altogether a younger and less indurated character than those of other districts where they are directly related to the central or fundamental gneiss;

(3) Frequent strong signs of some sort of bedding, even down to the gneiss; marked examples of which were observed in the Reussthal and on the Maloja;

(4) Signs in some cases of subsequently-induced subordinate structural characters, such as cleavage and the deposition of veins of secondary quartz.

Of the rapidity with which the younger phyllite series undergo degradation by atmospheric agents in weathering, as compared with the older schists, I have noted several illustrations, in the work done by great downpours of rain in the mountains. For example, a storm during the previous night

had swollen the waters of the Nolla which rolled like a sea of black mud into the Rhine at Thusis; and about half-an-hour from that place the road to the Schyn Pass was found blocked by a vast mass of debris from the same black slaty rocks of the phyllite series.

Again in wandering in the Bavarian Alps, in the Vorarlberg, and in the Salz Kammergut on the north side of the Alps; in contrasting the calcareous series of the north side of the Ennsthal with the micaschists on the south side; in contrasting the Triassic strata of the Dolomite Alps to the south of the Pusterthal with the phyllites the mica schists and the gneiss of the central chain on the north side of that valley; and lastly in following the latter up the valley from Lienz to Heiligenblut and on to the Gross Glockner, one could hardly help reflecting upon the slight petrological change wrought in the later and undoubted sedimentary series with the enormous degree of 'metamorphism' which the rocks of the central chain had undergone, on the assumption that they owe their present character altogether to 'metamorphism.' This is especially striking when one compares, in hand-specimens or in field-sections, the Triassic limestones on either the north or south side with the massive calcareous schists (Kalk-glimmer-schiefer) which are seen interbedded with the chloritic schists and above the mica-schists of the Glockner,* (exposed in good fresh sections below the Glockner House.) The slight splintery kind of incipient cleavage, which has given to the 'Dolomites' on the south side and to ranges like the Donner Kogeln on the nor thside above Gosau their weird and grotesque appearance (as the result of the peculiar mode of weathering caused thereby in these rocks), and is well seen in hand-specimens of some of the Hallstatt limestones (which have undergone a certain amount of metatropic change in places into marble),—is altogether a different thing from the marked foliation observable in the Kalkglimmerschiefer of the Glockner district.

Reflecting on such facts, one found oneself gradually driven of necessity to conclude that the *mere pressure*, which has wrought so small a petrological change in the sedimentary strata of the flanking ranges of the Alps, *cannot be accepted as the cause of the petrological characters peculiar to the gneiss and schists of the central chain.* Some of the extreme results of such pressure are seen in the true cleavage of the Glarus slates (Dachschiefer) where the Eocene strata have been subjected to excessive pressure between two great overfolds of the older strata (Doppelfalte,)† and of the slates of some of the secondary formations of the Alps (as in the case cited in this note and other well-known instances); and I have a specimen of rock from the Schafberg above Pontresina (probably originally a quartrite) in which a rough cleavage-foliation cuts right across the bedding.‡ No one would contend

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* Position assigned to them by Gümbel. (*Anleitung zu wiss. Beobach. auf Alpen-reisen*, fig 52), cf. Bonney, (Q.J.G.S. vol. xlv, pp. 86-90).

† See Heim, *Mech. der Gebirgsbildung*, (*Atlas*, prof. v, vi, vii, viii.) In the text of that work (Bd. i, pp. 143-146) Heim has given in full the palæontological and lithological evidence of the Eocene age of these Glarus slates.

‡ The "quartzite in which a rough cleavage-foliation" has been developed appears to be a schist, composed of alternate layers of a brownish yellow mica and quartz, the mica occurring in very thin layers, little more than films, under the microscope. The quartz has the 'mosaic-like' texture, and some of it appears to have taken the form of tridymite, as if the rock had been partially fused. On the other hand many of the quartz grains strongly suggest a clastic origin for the original rock. These facts and the proximity of the rock to the granite seem to suggest that the character of the rock is the result of contact-metamorphism. It is also intimately associated in the same quarry with a schistose chloritic rock, which as seen under the micr. must I think be certainly considered a product of contact-metamorphism. There is an accessory mineral scattered pretty

I think, that the lower degrees of metamorphism which these rocks exhibit can bear any comparison with the characters of the schists and gneiss of the oldest Alpine series. To the observations in the Alps which have been sketched in this note, may be added others, followed up however in less detail, in the Erzgebirge, in the Dresden and Freiberg country, in Thüringen, and elsewhere. Referring these to the horizontal ground-plan of those parts of Europe as indicated in the maps of Von Dechen, Studer and Escher, and Von Hauer, one's mind has been educated to appreciate the simplicity of the idea which runs through the tabulation of the rocks by those eminent geologists. In this way one has been led to view the crystalline archæan rocks, as nothing more nor less than *portions of an original earlier crust of the globe*, which have worked their way up through the later crust, under the influence of those great lateral strains, to which the crust as a whole has been subjected in the contraction of the terrestrial mass from loss of heat; while there is no ground for supposing that in such movements these so-called highly metamorphosed rocks have suffered a higher degree of change than those minor phases (metataxic chiefly) which can be seen in the undoubted sedimentary strata that have partaken in the same movements.* It follows further from this that the *crystalline archæan rocks (gneiss, and schists, and phyllites) owe their mineral character (their essential morphology) to causes independent of the subsequent movements in which they have taken part; that is to say, to the conditions under which they were originally deposited.*

NOTE I. (cf. p. 10).

On Serpentinization, &c.

The following note (based on data furnished by Rammelsberg's 'Mineralchemie') may be useful as illustrating the chemical changes involved in serpentinization and some allied processes:

OLIVINE—$FeO + SiO_2 + H_2O$ = **SERPENTINE.**

[MgO, FeO.] [MgO (FeO) + H_2O.]

An isomorphous mixture of $\frac{1}{2}$-silicates of MgO and FeO, by losing a good portion of its ferrous oxyde and taking up silica and water, becomes essentially a hydrous silicate of MgO.

AUGITE – $CaO + H_2O$ = **SERPENTINE.**

[CaO, MgO, (FeO, Al_2O_3)]

HORNBLENDE – CaO (– FeO) + H_2O = **SERPENTINE.**

[MgO, CaO, FeO (Al_2O_3)]

In the second and third cases some Al_2O_3 would probably remain as an impurity mechanically mixed with the mineral. Kalkowsky ('Lithologie,' p. 208) remarks on the serpentinization of amphibolites which are poor in Al_2O_3, the hornblende passing into a clear green fibrous magnesia-hydrosilicate.

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generally in well-individualized particles through the specimen; and these are generally elongated in the direction of the foliation-planes. In places it lines the walls of, and in one place fills up, a branching fissure or former crack in the slide which runs obliquely across the foliation, and is filled for the most part with secondary SiO_2. The mineral is quite opaque and is probably magnetite. Glassy quartz is visible macroscopically in thin folia. Conversion of quartz into tridymite requires the temperature of a porcelain-furnace, to melt it into opal that of the flame of the oxyhydrogen blow-pipe (Rammelsberg, *Mineralchemie*, p. 162.)

The statement of Kalkowsky which has been already quoted (*Lithologie*, p. 270), seems to apply to this case; so that it would furnish an example of what that writer terms 'Quartzitschiefer.' It is rather a nice question perhaps whether the case is one of anything more than 'cleavage-foliation.'

* Mr. Marr has arrived at a similar conclusion in the case of the so-called argillaceous schists at Ilfracombe, where he recognizes 'sedimentary rocks possessing all the mechanical peculiarities of normal schists, without any great amount of chemical change.' (*Nature*, vol. xxxvi, p. 591).

GARNET (resp. Thongranate) – $CaO + MgO + H_2O$ = **SERPENTINE.**
[$Al_2O_3 + FeO$ or CaO] [+ Al_2O_3 as an impurity.]

Further—

Serpentine – FeO = Talk.

Serpentine – $MgO + O$ = Hæmatite.

On the other hand, with a different rock-environment furnishing a different set of minerals in solution, the chemical changes set up would follow a rather different direction, thus:

Olivine – $FeO + H_2O$ = Chlorite.

Olivine – $MgO + H_2O$ = Chlorophaeite.

Augite (in some cases) – $CaO + H_2O$ = Chlorite.

Chlorite (with slight chemical changes) becomes Serpentine.

With the common conversion of olivine (one product of dry fusion) into serpentine by hydrochemical action it is interesting to compare the relation which Pectolite bears to another product of dry fusion, the normal silicate of lime, Wollastonite ($CaSiO_3$).

On the other hand, from a series of anhydro-chemical reactions of a mineral magma upon the normal silicate of magnesia, Enstatite ($MgSiO_3$), such minerals as Bronzite, Hypersthenite, Diopside, may have originated (see Rammelsberg, *op. cit.*, pp, 379-389.)

Roth *(Allgem. u. Chem. Geol.*, Bd. i, p. 67*)* enumerates the non-aluminous silicates Olivine, Enstatite, some Augites and Hornblendes, Diallage, Chondrodite, as the more important minerals convertible into serpentine (and its allied minerals), which often replaces them in pseudomorphs. (cf. Note N.)

Note K.

Pfaff *(Allgemeine Geologie als exacte Wissenschaft)* has to some extent anticipated me with reference to the conditions of the earth's surface, under which the earliest 'rocks' must have been formed; conditions which I have in this work deduced independently from known chemical and physical facts and principles. See also British Association Report, Birmingham Meeting (1886), pp. 658-60. Pfaff's doctrine must be stated in his own words, p. 143—

"In den früheren Zeiten war stets eine Gränzlinie der Tiefe vorhanden, über welche hinaus *kein flüssiges Wasser* gelangen konnte. Diese Gränzlinie rückte mit der fortschreitenden Erkaltung immer weiter nach abwärts, und von einem gewissen Zeitpunkte an trat dann das Verhältniss ein, welches wir jetzt finden, dass nehmlich flüssiges Wasser bis zu dem heissflüssigen Erdkerne selbst dringen kann."

Note L. (cf. pp. 21, 92).

The genesis of the Diamond and of Graphite.

In the year 1880 Mr. Hannay announced that he had succeeded in making real diamonds by the reducing-action of sodium upon hydrocarbons at high temperature and pressure.* Assuming that the specimens submitted by him to Prof. Maskelyne were made in this way, much new light would seem to be thrown upon the obscure process whereby the diamond is formed in nature. The presence of heat and pressure in the depths of the earth's crust sufficient for the purpose will be readily admitted; the real difficulty which remains is to find a sufficiently energetic electropositive metal to do the work of reduction, by taking up the hydrogen of hydrocarbons. There is some considerable difficulty on general grounds in postulating the presence of the alkalimetals, but less difficulty in considering that the metals of the alkaline-earths might be present. Looking however at the position of *magnesium* (in the electropositive order of the metals), it seems probable that this widely-distributed metallic base of silicates might be regarded as the most likely metal to have

* See *Nature*, vol. xxii, p. 255.

effected the reduction; and this hypothesis is strengthened by some of the known chemical properties of magnesium. In this way I had, on purely theoretical grounds, anticipated the conclusion at which Prof. H. Carvill Lewis arrived. (See Brit. Association Report, 1887.)

Messrs. Roscoe and Schorlemmer (*Treatise on Chemistry*, vol. i. p. 582) mention the occurrence of dark spots in diamonds, which Brewster considered to be cavities, but which Sorby has shown to consist of "small crystals of much lower refractive power than the diamond itself." May not these be 'belonites,' and as such traces of an incomplete metatropic change of the carbon? An affirmative answer to this question is certainly suggested by the further fact noted by the same writers, that "Goeppert has noticed in certain diamonds the occurrence of a cell-like structure resembling that obtained when a jelly undergoes solidification." To the statement of Roscoe in his *Elementary Chemistry* (edition 1887, p. 75), that the diamond "cannot have been produced at high temperature, because, when heated strongly in a medium incapable of acting chemically upon it, the diamond swells up and is converted into a black mass resembling coke," we must demur, as the effect of the simultaneous action of great pressure is ignored.

With this we may connect *the occurrence of Graphite in the earliest rocks.*

Some stress has been laid upon this fact by different writers, as indicating carbonization of organic matter; and then the further inference is drawn that the rocks in which it is found must have once been sites of vegetable growth, or derived from other rocks which were. If this were a *necessary* inference it would certainly lend strong support to the extreme theories of metamorphism which have been maintained in some quarters. We must see if experimental chemistry can throw any light on this subject. (1) We know perfectly well that CO_2 can be reduced to elementary carbon in the purest form by hot alkali-metals, and even by heated metallic magnesium; but this introduces the further difficulty of explaining how such metals could have existed in any quantity in the free state, since the alkali-metals must have been among the earliest elements to undergo oxidation; and the avidity with which they attack water to form hydroxides makes it still more difficult to understand how they could have existed in the free state at that stage of the earth's evolution at which we consider these rocks, in which graphite occurs, to have been formed. But (2) we know also that at high temperatures elementary carbon can combine directly with hydrogen, a fact which we frequently call into play in the laboratory in the synthesis of acetylene gas (C_2H_2) at the temperature of the electric spark-stream. Here then we have a clue to the mystery. Anyone who is familiar with the chemistry of the hydro-carbons will, I think, be prepared to admit that such bodies might have been formed in some quantity under the physical conditions existing at the time when the archæan rocks were formed.* Given the presence of such bodies in the nondifferentiated mass from which the materials of these rocks were deposited, how could they be reduced to elementary carbon by the abstraction of their hydrogen? In two ways: *(a)* by direct dissociation of the higher hydro-carbons; marsh-gas and olefiant gas *(e.g.)* undergoing this change before our very eyes in the eudiometer under the influence of the high temperature of the spark-stream, and depositing carbon on the terminals with such rapidity as to repeatedly bridge over the interspace and close the circuit; *(b)* by actual reduction under great pressure and high temperature, by the chemical action of strongly positive bases. Perhaps the nearest approach to the physical conditions under which we conceive graphite to have been formed in the fundamental rocks is found in the heated interior of the ordinary fire-clay retorts in use in gas-works. "A very pure and dense

* Do we know absolutely anything as to the origin of the vast stores of hydro-carbons in the oil-fields of America, or of that of the greater stores of the Baku region? Are there any incontestable facts which go to establish an organogenic, in contrariety to a cosmic, origin of those hydro-carbons?

variety of carbon is found in the roof of old gas-retorts, where it has been gradually deposited by the action of the high temperature upon the [hydro-carbons of the] coal-gas which was passing out."* To the influence of high temperature here mentioned I should consider "contact-action" of the heated porous body of the wall of the retort as an important aid towards dissociation and consequent deposition of the carbon.† All this is quite conceivable as taking place at an early stage of the evolution of the earth.

From these considerations it appears that there is no necessity for regarding the presence of elementary carbon in the form of graphite in the archæan gneisses and schists as indicating pre-existing organic matter.‡

The interpretation of the presence of graphite in the archæan rocks as indicating pre-existing vegetation was admitted even by Prof. Möbius (*Der Bau des Eozöon Canadense*, § ix) so late as the year 1878. His words are: "Vielleicht rührt der Graphit der Urgneissformation von Organismen her." Kuntze in commenting adversely (*Nature*, August 28, 1879,) on Möbius' conjecture as to the origin of graphite, denies its phytogenic origin in the Laurentian rocks, though he does not take quite the line of argument which I have in this Note. His objections are of a general nature; and his strongest point is the absence of water in the minerals of the archæan rocks, though perhaps he overstrains this point a little. Of the existence of Kuntze's letter (though I may have read it at the time of its appearance and forgotten it) I was quite unaware while engaged in working out the view which I have ventured to put forward as the probable explanation of the occurrence of graphite: and I only came upon it in looking up afterwards such notices of the *Eozoön Canadense* controversy as might have appeared since 1878. This was in April, 1888. So convincing to some minds has the presence of graphite appeared as an indication of the pre-existence of vegetation on the Earth, that in the Report of the Smithsonian Institution (1869) quoted by Sterry Hunt (*Chemical and Geological Essays*, p. 302) the presence of graphite even in aërolites is said to "tell us in unmistakeable language that these bodies came from a region where vegetable life has performed a part not unlike that which [it] still plays on our globe!"

Lockyer's experiments on meteorites (see *Nature*, vol. xxxiv, p. 280) are of great theoretical interest in this connection. Experimenting on meteorites in a vacuous space with a *low-temperature* spark-stream he obtained "the same spectrum of hydro-carbons which Huggins, Donati, and others have made us perfectly familiar with in the head of a comet." This seems to support the hypothesis advanced in this Note as to the probable existence of hydro-carbons in the archæan atmosphere of the Earth.

With a *high-temperature* spark-stream he obtained in a similar manner the hydrogen spectrum, without the carbon spectrum. Is not this due to the temperature of the spark-stream being in this case above that of the dissociation of the hydro-carbon while below that required to give the spectrum of incandescent vapour of carbon? If Mr. Lockyer will repeat this experiment for some time and examine the apparatus afterwards for amorphous carbon-dust, and also examine the spectrum of the spark as it passes between two *carbon-points* in the synthetic formation of acetylene gas, some interesting results may very likely be obtained tending to give a definite answer to this question.

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* Williamson: *Chemistry for Students*, § 54.

† See *Chemical News*, vol. liv, No. 1402, where I have discussed this subject at length.

‡ Since this note was written, it has been announced in *Nature*, (Dec. 15th, 1887), that "carbon has been found between the laminæ" of the great mass of meteoric iron which fell near Cabin Creek, Johnson's County, Arkansas, March 27th, 1886. Graphite "identical in properties with iron-graphite" was also identified by Berthelot in the meteoric mass which fell at Cranbourne near Melbourne in 1861.

Etheridge (Pres. Address, 1881, p. 26) remarks: "The presence of graphite in large deposits, occurring both in beds and veins in the Laurentian rocks, clearly determines that its origin and deposition were contemporaneous with the mass of the containing rock; the graphite, again, is associated with calcite quartz, and orthoclase."

Sterry Hunt says *(Ch. and Geol. Essays*, p. 216*)*: "Graphite, which itself encloses apatite, is found included alike in quartz, orthoclase, and pyroxene, and in calcite, in such a manner as to lead us to conclude that the crystallization was contemporaneous with that of all these minerals." The assumption of the necessarily phytogenic origin of graphite seems to have found pretty general acceptance with recent writers of text-books (see Geikie, p. 639).

In this Note I have done no more than drop a hint that the dissociation of hydro-carbons (previously formed as they may have been by direct combination of hydrogen with glowing carbon-vapour) was greatly facilitated by the *contact-action* of solid portions of a crust formed locally on the surface of the magma, but still at a high red-heat temperature, from a comparison of those instances which I have discussed in the paper referred to in the note, and which are within the range of our ordinary laboratory experience. I venture to go further now, and to make the suggestion that this has been *the main agency concerned in the great deposits of archæan graphite.* All that we should seem to require would be the presence of slaggy solid portions of the Earth's surface sufficiently cooled to be below the temperature of the evaporation of carbon, which must be enormously high. Graphite thus becomes a rough indication of the stage at which a hard but hot crust began to form locally on the surface of the magma; a beginning (that is to say) of the passage of the Earth from the 2nd to the 3rd of Zöllner's phases.

On May 12th, 1888, I subjected this theoretical view to the test of experiment. A piece of ordinary combustion glass tubing about 8 inches long was drawn out at one end nearly to a point, so that a continuous flame of gas might burn at it, to answer the double purpose of guaranteeing that no atmospheric air passed back into the tube at any stage of the experiment, and giving at the same time a rough index of the rate at which a stream of gas passed through the tube. The tube was filled with fresh-broken and calcined lumps of pumice, and the wide end fitted with an ordinary wooden cork and connexion-tube. This was then attached to an ordinary flexible tube for the supply of common coal-gas. The coal-gas was passed through and ignited at the other end. The tube itself was then raised to a dull red heat by a Bunsen-flame about 6 inches wide; and as I stood and watched it I had the satisfaction of seeing the pumice-fragments gradually coated over with a black deposit, possessing the dullish metallic lustre of graphite. It is impossible that this could have been formed in any other way than by the dissociation, through the contact-action of the heated porous fragments of pumice, of the hydro-carbons of the coal-gas.

In order to further verify this, the tube was detached from the coal-gas supply, and attached to a gas-holder full of oxygen, with the intervention of drying-apparatus. The oxygen was passed for a few minutes (cold) through the tube and then through lime-water without any visible result; then a narrow zone was heated with a flame of a spirit-lamp while the oxygen continued to pass. The combustion of the carbon in the heated zone of the tube was soon proved by the free deposit of carbonate of lime from the lime-water, and in two or three minutes those pumice-fragments which were exposed to the heat of the flame were perfectly cleaned to their native whiteness. The theory of dissociation by contact-action thus receives visible demonstration; and the application of the theory to the production of graphite from hydro-carbons seems to be fully warranted.

I have since repeated the experiment on a larger scale, and have a tube full of pumice-fragments coated over with carbon, more than 3 feet in length. In this experiment I observed the deposition of elementary sulphur, from the

dissociation of the sulphur components of the coal-gas, along with the deposition of the carbon.

Dissociation by contact-action at a pretty high temperature of portions of the solid crust upon hydro-carbons may be regarded, I think, in the light of such evidence, as a sufficient explanation of the occurrence of graphite in rocks of undoubted archæan age; this therefore need no longer have a phytogenic origin ascribed to it. As a simple corollary to this, it follows that later, say in the stage represented by the Huronian of Prof. R. D. Irving (*op. cit.*) we should expect to find, under the conditions which he has deduced from his masterly studies of that great group, that, the temperature being too low to induce dissociation, the hydro-carbons would be deposited. Such 'hydro-carbonaceous' material need not therefore be regarded (as he supposes) as 'organic matter' (p. 373) any more than the archæan graphite; and so his argument on this point seems to break down. As for the iron-carbonates of that group, they simply tell us that (on the assumption of their contemporaneous origin) in the Huronian period of the Earth's developement, the temperature in that region was not above the dissociation-temperature of carbonate of iron.

While this was passing through the press, Mr. T. Davies of the British Museum of Natural History was good enough to direct my attention to some very interesting specimens of graphite from Siberia, which are contained in the National Collection, as exhibiting what had appeared to some to be traces of 'organic structure.' From an inspection of them which through Mr. Davies' obliging courtesy I have been able to make, I am convinced that there is no trace whatever of organic structure in them; and that the phenomena which they present can all be explained as the developement of a crude prismatic structure caused by compound cleavage due to compression combined with 'Answeichungsclivage' accompanied in some cases with a certain amount of crushing along the transverse shearing-planes. See further paper by the author read before Sec. B. (Brit. Assoc., 1888) and published *in extenso* in the *Chemical News*, No. 1505.

NOTE M. (cf. p. 59.)

Fossil-evidence of Extension in direction of Cleavage-dip.

The observation that fossils undergo frequent distortion in this direction, and thus afford direct evidence of it in the genesis of a *slate*, is a very old one. Much has been made lately of the case cited by Heim (see Marr, Brit. Assoc., Manchester, 1887, reported in Nature, *loc. cit.* Note H.) of Belemnites in the Jurassic slates of Switzerland being parted asunder transversely and the zones interspaced with crystalline calcite. But facts of this nature are not new.

In the Museum of the University of Zürich, as Cotta informs us in a letter from that city dated 1849, there had been collected even then a numerous suite of Belemnites from the Lias-formation, which for the most part had been so altered by squashing (Quetschung) that they could only be recognized as Belemnites by comparison of many examples. These Cephalopods (V.C. goes on to say) "lie in a dark clay-slaty rock, and are for the most part, as it appears, torn assunder (zerissen) into single parts through the squeezing and stretching of the rock in the direction of its cleavage (Schieferrichtung), or they have been stretched out into knotty staves which have now only a faint resemblance to Belemnites. *The interspaces* (Zwischenräume) *in the former cases have been filled with slate.*" (cf. p. 59 of this work, also Note T. *infra*).

NOTE N. (cf. p. 78).

Von Cotta is precise in his account of the observations which he made of the serpentinization of granite. He must tell his own tale:

"At Predazzo we happened for the first time to examine one of the places where the limestone and granite are in contact. We selected the rock-wall of the Canzacolli. Not without fatigue we climbed up on the boundary between

the granite and the limestone as far as the great quarry, in which beautiful white marble is obtained, and then still further upwards to where the limestone spreads out over the granite. As far as the marble-quarry we found the boundary everywhere clearly defined and quite sharp. In many instances the granite branched out in vein-like (gangförmig) processes into the limestone, and, (what is especially noteworthy) these at first (at their origin from the *massif*) undoubtedly granite veins become, as they penetrate further into the limestone, more and more talcose, and very soon pass over into undoubted serpentine-veins, by which the marble moreover is often sharply intersected, as seen in large fragments. The serpentine-veins had been previously observed by Fuchs and Petzholdt; but that they spring [as apophyses] out of the granite, and still consist in part of granite, had no one, so far as I know, before noticed." ("Die Alpen": Weigel, Leipzig, 1881, pp. 196-197).

To leave nothing wanting in the precision of his statement, V. Cotta figures the exact position of the spot where the observation was made, and adds a coloured drawing to shew the exact relation of the serpentine both to the granite and the marble.

Note O. (cf. pp. 65-68.)

The Moon's Surface.

Does not Prof. R. S. Ball's theory as to the origin of the Moon suggest an explanation of the result to which Zöllner's investigations have led, that the higher parts of the Moon's surface (those which appear to us illuminated) are composed of materials which on the Earth would be regarded as among the whitest of substances? Can we not follow rationally the Moon in its earlier orbits revolving round the Earth for some time within the limits of the terrestrial atmosphere, then far more extensive than now, if we may judge from the proportionate extent of the present atmosphere of the Sun? And does it not seem highly probable (by all physical considerations) that in the outer perisphere steam and CO_2 (and perhaps other acid gases) would be much more abundant than in the lower strata of the Earth's atmosphere, while the surface of the globe itself was still in a glowing liquid state? And would not the comparatively rapid cooling of the smaller lunar mass admit of condensation of carbonated water from the outer terrestrial atmosphere upon the surface of the Moon at an earlier period than that was possible on the surface of the Earth to any very general extent? Have we not here the factors needed for converting the silicates at the surface of the lunar mass into carbonates and free silica to furnish a white rocky outer crust to the Moon? In the absence of a permanent lunar atmosphere and of permanent lunar waters, owing to the greater attractive power of the Earth's mass, and the consequent re-evaporation of any previously condensed free water on the Moon's surface, as that orb passed beyond the limits of the Earth's atmosphere, would not such an early crust remain intact (except where broken through by igneous outbursts) from the absence of disintegrating agents?

Tidal waves. Of course, while the Moon remained unconsolidated *earth-tides* must have been produced in it as the counterpart of the 'lunar tides' produced in the Earth; and these would be so much the greater in proportion to the greater mass of the Earth at any given distance. May not such tides in the Moon in its earlier individual existence along with the more rapid cooling of the smaller lunar mass, explain in part the prodigious size of the mountains as compared with the mass of the Moon itself? May not the 'rills' and 'walled plains' be in such way connected with the earlier 'earth-tides' in the Moon? Again, the appearances presented by such huge lunar volcanoes as Copernicus and some others do not suggest the general radial arrangement of volcanic outflows from a centre (as in terrestrial volcanoes) but a certain rough parallelism in the ridges which bound this and the adjacent craters, as if the craters were but eruptive openings through the crests of great tidal waves in the partly consolidated lunar crust.

Perhaps the surface of the moon teaches us more than anything else what the earth's surface was like in the earlier stages of the formation of a solid rugged glowing outer crust, at a time when oceanic waters had not as yet begun to condense upon it. It has been pointed out already that the atmosphere of the Earth was then probably much more extensive, and that in the outer regions of it the earliest water (steam), CO_2, and perhaps other gaseous oxides would be formed. The action of these upon the surface-materials of the moon has been also suggested as probable; the only assumption (and that not an extravagant one) being that the Moon's surface advanced to some extent in the process of solidification while as yet within the limits of the Earth's atmosphere. If this were so, it is almost certain that much steam would be included mechanically in those slaggy portions of the Moon's surface which solidified first, giving them a more or less *pumiceous* character. The consequence of this would be that these solidified portions with their included steam would be specifically lighter than the lunar magma in which steam was not in this way imprisoned. They would therefore float on the magma, just as pumice floats on water when its cavities are air-charged. Now, if we extend the idea of solidification setting in at the crests of tidal waves of the Moon's magma (which on physical grounds seems highly probable not merely for the reason suggested in this work, but from the fact that the mass of a wave would present a larger surface relatively to its mass for loss of heat both by radiation and by convection-currents in the adjacent terrestrial atmosphere,) and consider at the same time how waves of the sea become charged with air at their summits so as to be converted into foam, we perhaps have the clue to the striking and peculiar features presented to us by some of the lunar mountain-ranges, especially that of the Lunar Apennines (see Nasmyth and Carpenter's work, *The Moon*, Plate ix). On Plate xi of the same work is shown a series of mountain-ridges, having such a marked parallelism with one another as to be strikingly suggestive of a series of 'waves and troughs.' In the central one is the great lunar volcano Triesnecker. In the broad valley on the left of this in the photograph is a series of fissures ('chasms'), in some instances nearly a mile wide and 100 miles long. May not these, running, as they are seen to do, approximately parallel to the mountain-ridges, be a series of huge shrinkage-cracks, owing to the adhesion of the material as it solidified to the parts already solidified, thus repeating essentially the phenomena of joints, of columnar structure, and septaria? In the lunar region about Aristarchus and Herodotus the same 'wave-and-trough' structure is so remarkably well seen that the general parallelism of the ridges seemed to the authors of the work to call for special remark (p. 84). Fissures with a similar relation to the mountains are shown here and in the broad valley by the side of the Lunar Apennines in Plate ix. Other instances of 'wave-and-trough' parallelism (though less marked) are shown in the frontispiece of the same work (Gassendi), in Copernicus (Plate viii), in Mercator and Campanus (Plate xv); while in Plates x and xii a certain general linear arrangement of the volcanoes, such as Judd's papers on the Lipari Islands have taught us to refer to lines of weakness in the Earth's crust, may be observed.

Taking the mass of the Moon as $\frac{1}{80}$th that of the earth, it is manifest that, at the distance of the Moon from the Earth at which the rigid crust began to form on the former, the tidal waves would be much higher on the Moon than on the earth; and owing to the greater mass of the Earth the conditions required for incipient solidification would be reached in the process of cooling by radiation much later on the Earth than on the Moon; so that by the time incipient solidification at the surface of the terrestrial magma became possible, the Moon's distance from the Earth would have so far increased as to lead to a very considerable lessening of the degree of tidal action produced by its attraction on the terrestrial magma. From these two considerations we should expect that, if such phenomena as are presented by the Moon's surface repeated themselves at a later stage at the surface of the Earth, the altitude

of the ridges of elevation thus formed would be so much less on the Earth than on the Moon that they might have been completely obliterated by the action of subsequent oceanic tides. The argument for the *flotation* of the first solidified portions upon the liquid magma on account of the inclusion of steam applies to the case of the Earth as well as to that of the Moon.

NOTE P.

The case of the North-Western Highlands of Scotland.

In this work a suggestion is thrown out as to the possibility of tidal waves initiating some of the lines of upheaval of the Earth's crust. In a somewhat qualified form there appears to be some sense in that idea. We can conceive the early formation of a solid thin crust corresponding to Zöllner's third phase of developement. Now for a long time this must have been so thin that *the tidal waves formed by the Moon's attraction on the enclosed magma must have caused frequent ruptures of this crust, with eruption of portions of the enclosed magma.* This could scarcely happen without inducing *foliation* in the intrusive masses, from the shearing produced by the sliding movement of the solid portions of the outer crust, between which they were intruded; but what right have we to speak of this as even 'pre-Cambrian metamorphism,' unless we so qualify the phrase that the term 'metamorphism' becomes meaningless in so far as any definite sense has been hitherto attached to it in geological literature? This point receives illustration from the remarkable facts which the Director-General of the Geological Survey of Great Britain laid before the Geological Society of London on April 25, 1888, (see Abstract of Proc. No. 522). He describes, as occurring in archæan times in the N. W. Highlands of Scotland,

(1) eruption of basic igneous rocks;
(2) the developement of a rude foliation in these masses probably by mechanical means, and their arrangement in gentle synclines and anticlines;
(3) injection of igneous dykes into the original gneisses, consisting of—
  (*a*) basalts (? dolerites),
  (*b*) peridotites and picrites
  (*c*) microcline-mica rocks,
  (*d*) granites;
(4) mechanical movements giving rise to lines of disruption; and producing
(5) changes of the more basic igneous rocks into schists, and of the granites into gneiss.

Surely this looks like the kind of work which we should expect tidal waves in the magma to do in the earlier stages of the formation of a solid external crust, as I have suggested above. The penetration of the fissures and junction-planes by such water as was locally condensed under pressure would give all the conditions required for such paramorphic changes as are indicated.

The remark, that "there is overwhelming evidence to prove that all these various changes had been superinduced on the archæan rocks in pre-cambrian time," is of the highest value as testimony from such an authority; adding as it does one more striking and incontestable instance in confirmation of the generalization of Credner's quoted in this work (p. 17) "that the process of metamorphism so-called, which distinctly characterizes the pre-Silurian (pre-Cambrian) formations, was at the entry of the earth into the Silurian (Cambrian) period already completed."

We have in fact one more chapter added to the record of the kind of work which was going on in the developement of the present Earth during Zöllner's 4th phase of developement, the pre-oceanic stage of eruptive energy.

Evidence is brought forward of 'regional metamorphism' accompanying at a later period the great terrestrial displacements which occurred in the mountain-building stage, such as

(i) formation of new divisional planes in the Cambrian gneiss;
(ii) conversion of grits and shales into 'schists';

(iii) conversion of Silurian quartzites into 'quartz-schists';
(iv) crystallization of limestones;
(v) foliation produced in igneous rocks.

On this the question may fairly be asked if the schists (ii) are really anything more than *phyllites*, implying at the most mere 'cleavage-foliation;' also if the production of 'quartz-schists' is intended to indicate anything more.

Kalkowsky describes such features of 'Quartzitschiefer.' "Viele Quartzite sind deutlich und zum Theil dünnschiefrig, und werden deshalb Quartzitschiefer genannt. Diese vermögen auch transversale Schieferung anzunehmen, wodurch sie bisweilen in zweischneidige, messerklingenförmige Griffel zerfallen." (*Lithologie* p. 270).

Upon the whole (with the exception of the dubious case of the 'schists' (ii), which evidently need more definition, little, if anything in the way of paramorphic change seems to be implied in the facts brought forward. Such changes as have occurred in this later mountain-building stage appear to be merely of a metatropic and metataxic character (mainly the latter). What the distinguished author may mean by the "*chemical* effects of mechanical movement" requires further explanation than is given in the abstract. From the information before us they appear to be little, if anything, more than physical and mechanical changes.

A perusal of the paper* in detail leads to such reflections as the following,

(1) What are these and similar 'eruptive dykes' of basic igneous rock of archæan age? Are they 'anogenic'? or are they 'katogenic'? Intrusive† they undoubtedly are. But are they composed of highly basic materials formed by reactions of the heavy metals (iron, &c.) of the barysphere or metallic kernel upon the earliest silica and silicates deposited, and squeezed up from below through planes of weakness by the force produced by the ever-increasing weight of the growing lithosphere? Or are they on the other hand infillings from above of huge divisional planes of contraction of the yet glowing but partly-consolidated granitoid and gneissic masses, by such more

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* See Q.J.G.S., vol. xliv, August, 1888, "First Report of the Recent Work of the Geological Survey in the North-West Highlands of Scotland." On reading the paper I ventured to write the criticism which here follows. This I intended to read before Section C of the Brit. Assoc., but it was rejected by the organizing Committee of that section on the ground that it was "too much of the nature of a review," and that the section "could not judge of it without the original paper before them." This of course was reasonable; and I have not thought it worth while to enquire into the constitution of the 'Organizing Committee.' Having been favoured however, while at Bath, with a copy of the 'Essays on the Crystalline Schists' contributed by eminent foreign geologists for the International Congress which met in London on September 17th, 1888, I was struck with the emphatic way in which Prof. Heim's remarks in the concluding paragraphs of his Essay gave support to my contention; so much so, that on the 16th of September, I sent the paper off, just as it was first written, to the Editor of the Geological Magazine. He kept it for two months, and then returned it to me stating in usual editorial phraseology that "want of space had prevented his being able to publish it." This statement I have found difficult to harmonize with his *embarras de richesse* in the matter of space some three months previously. Perhaps the audacity of this criticism took Dr. W's breath away, or he generously thought to save me from the discomfort of having to confront the awe-inspiring visage of the Sphinx of Jermyn Street. Strengthened in my conclusions by an examination of the rock-specimens exhibited last September, I publish it here in the form in which it was originally written.

† 'Intrusive' (that is to say) as distinguished from segregative. I do not believe in the pretty hypothesis of 'segregation-planes,' which a few geologists have evolved out of their imagination.

basic materials as are found everywhere, where the normal developement can be studied, in the overlying archæan crystalline schists? We look with the keenest interest for evidence to guide us in the choice of these alternatives. At present not a fragment has been furnished.

Our thanks are due to the authors of the paper for the thoroughness of their observations on the igneous intrusive rocks of the area dealt with; a thoroughness such that it has added one more region of the earth's surface to those in which observations of a similar kind had been previously made. The conclusions of Suess* from observations made in the whole region of the Austrian Alps and the Carpathians, as to the '*passivity*' of igneous intrusive rocks in the process of mountain-building, were amply confirmed ten years ago by Heim† for the Central Alps, for the intrusive porphyries, melaphyres, granites and syenites alike; and now the classical region of the N.W. Highlands has been made to bear testimony to the far-reaching truth of Suess's induction.

(2) Reference is here made to the passage "Reflecting on these facts .... originally deposited." (See Note H. in this appendix.)

These general remarks apply—I take it—with some modifications, to the 'metamorphosis' of the Cambrian and Lower Silurian strata of the N.W. Highlands of Scotland so powerfully described in this paper. The splendid work of Messrs. Peach and Horne and their colleagues has brought to light the extreme results which pressure-movements *locally intensified* are capable of producing. The most elementary principles of mechanics however are enough to show that such intensification of mechanical force and its transformation to a large extent into heat must be local. The less-yielding character of the rocks here than in those of the Tödi-Windgällen group has given us a predominance of crush-planes of thrust rather than of folding without fracture; otherwise the phenomena described seem to be essentially the same as those previously described by Heim; and they add nothing to the possibilities of which we were previously cognizant. *Such local effects of mechanical work cannot furnish a basis for a general theory of 'regional-metamorphism' applicable to the 30,000 metres of gneiss and schists of archæan age,* exposed to the light of day over large areas of the earth's present surface, and probably extending under the sedimentary formations round the whole sphere of the Earth. The author of the paper will have to pardon me therefore for saying that "It is [*by no means*] obvious [to those who look at the matter with a more critical eye] that the facts brought forward furnish a large amount of evidence in support of the theory *that regional metamorphism is due to the dynamical effects of mechanical movement acting alike on crystalline and clastic rocks.*"‡

(3) One broad result of their investigation is the *contrast* throughout the area dealt with, between the mechanical metamorphism of the Cambrian and Lower Silurian, and that *primary morphology* which characterises the Archæan rocks of this small area in common with that which they display wherever they have been examined in all the four quarters of the globe. Further, until a thorough microscopic examination is made, the above conclusion is at least premature. Better would it have been for science to have imitated the caution of Heim. *(op. cit.* Bd. II, p. 40, *et passim.)*

In the similar case of the Central Alps *(ibid, loc. cit.)*, "A very essential difference exists in their entire facies between the rocks of the central-massif and the younger sediments. In those schistosity (Schieferung) predominates, in these stratification (Schichtung); in those, steep, even, and regular position of the structural planes (Schiefer), in these, flexures, and even layers; those are formed for the most part of quartz and silicates, these of limestone and clastic materials (Trümmergesteine). The two different rock-groups can be

* *Entstehung der Alpen*, Abschnitt, I.

† *Mech. der. Gebirgsbildung*, Bd. II., p. 124.

‡ Q.J.G.S., *(loc. cit.)* page 438.

distinguished at a distance by their different forms resulting from weathering and denudation (Verwitterungsgestalten). The rocks of the central-massif have yet to be proved to be aqueous sediments ; many of them belong probably to the earliest lithosphere (Erstarrungskruste) of the Earth."

A perusal of the paper above referred to, (which has been hailed as an epoch-marking production in certain quarters)* leaves upon one's mind the impression that, with some modifications of detail, the above contrast holds good for those fragmentary remains of a great mountain-chain in the N.W. of Scotland.

(4) When we bear in mind that the *contraction of the outer crust* as the kernel became in course of time too small for it, on account of

(a) primarily, contraction due to continuous cooling and solidification ;

(b) secondarily, the loss of material from the interior through volcanic eruptions—

has given us (altogether independently of any theoretical views) the following stubborn facts† :—

(i) that the volume-proportion of the mountain-chains of the globe to the continents is such that the mountain-chains are quite subordinate phenomena ;

(ii) that the continental masses rise above the ocean of the present globe as huge broad pedestals upon which the mountain-chains rise as little ribs, adding about $\frac{1}{500}$ to their volume ;

(iii) that the *movements* of the Earth's rind which have differentiated the continents and the oceans have been different from those which have produced these mere wrinkles upon the great continental plateaux, though the force is primarily the same ;

we find it by no means possible to proceed so jauntily as some of our brethren of the hammer seem capable of doing, from the contemplation of such local, intensified, and extremely variable results of intense localization of mechanical force in the process of mountain-building, to the inference that the *agency* which has given us these special local results, in $\frac{1}{500}$ part of the elevated regions of the earth, has caused also the prevalent morphology of those vast regions of the remaining $\frac{499}{500}$, which still remain intact and proof against the agencies of denudation and degradation, or to leap such a mental 'Niagara' as is implied in any application of such results to the genesis of the great Archæan crystalline series of rocks.

(5) What are these huge 'thrust-planes' but planes of comparative weakness in the outer crust of the globe? They surely represent in the North-Western Scottish area the great overfolds of the Central Alps ; the results in both cases of the resolution of normal into tangential thrust, due to the contraction of the Earth's mass. Their magnitude strikes the mind of the observer with an astonishment, which—under its first spell—is almost bewildering ; yet they are but trifles when viewed as part of the whole sphere of the Earth. Take the case of "the disrupted rocks which must have travelled forward several miles from the east." ("Report, &c.," p. 429). A comparison of the sectional diagrams (figures 9-21) justifies us, I think, in regarding five miles as a liberal interpretation of this statement. If we allow 25,000 miles for the circumference of the Earth in palæozoic time we get

$$5 \text{ miles (measured circumferentially)} = \frac{5}{25000} = \frac{1}{5000}$$

of the Earth's circumference. This would represent a radial contraction = $\frac{2\pi}{5000}$ rather more than or little more than an 800th of the Earth's radius. Here however we are dealing with a maximum of circumferential movement. Had we the data for reducing this to the mean circumferential movement round a great circle of the globe, we should no doubt have to multiply our

* See the discussion on the 'Report,' Q.J.G.S., *loc. cit.*

† Heim *(op. cit.)* Bd. II, p. 236, *et. seq.*)

denominator by a factor of some hundreds. This rough calculation is put forward as a caution against hasty inferences from localized results of locally-intensified movements, as to the work done on a terrestrial scale.

In the slow contraction of the terrestrial sphere circumferential approximation of the more stable portions of the crust must come about. This will be admitted by everyone as an absolute necessity. What is to become of the intervening and less stable portions?

If we confine our attention to such a portion as has evidently been caught in the grip of two such massifs as the great Bohemian core of the European continent and the central massif of the Alps, we get a pretty clear idea of the main result, in the excessive over-folding of the outer flanking strata of the Eastern Alps, as long ago pointed out by Suess. A similar explanation I believe applies to the excessive folding of the Central Alps, when we view the position of these great disturbances with reference to the great massif of which the Schwarzwald and the Vosges are indications; while between these, and *vis-à-vis* of the great plain of Bavaria, there is no such excessive folding of the outer flanking strata of the Alps on the north side.

Substitute now for these comparatively soft and yielding strata, with their 'latent plasticity' (Heim)—allowing distribution of the energy of movement more or less through the mass—the more fragile materials of the Cambrian and Lower Silurian strata of the N.W. Highlands (as evidenced by the results observed); and we get thrust-planes taking the place of over-folding (in the main), with a consequent concentration of mechanical work along these planes, and as a further consequence intensification of the heat-energy into which the mechanical force is transformed. It is not unlikely—indeed, from the general conditions of the problem, it is likely—that the movements would be more rapid in the case of the displacements in the N.W. Highlands than in the more overfolded strata of the Alps; and this of course would intensify *(cet. par.)* the heat-energy developed. Exceptional conditions brought about in this way must give us exceptional results. Look at it how you will, in the light of broad general principles, *you cannot logically construct a theory of 'general regional metamorphism' out of such exceptional data obtained from a few circumscribed areas.*

(6) Lastly, a general unsatisfactory impression is left upon the mind that the scientific value of this first outline "Report" is much deteriorated by the vague indefiniteness with which the technical terms 'gneiss,' 'schist,' and 'slate' are used. Each of these words appears to be used in somewhat different senses in different parts of the paper. This much regrettable feature of it could have been avoided, and the scientific value of the production greatly enhanced, by the employment of a dual terminology, such as is so commonly in use among the leading continental geologists.

So far however from any desire to depreciate the value of the work of Messrs. Peach and Horne and their colleagues, I say again, that the scientific world owes them a great debt of gratitude; for I am convinced that the *facts* —rightly interpreted—will be found to have 'driven another nail into the coffin' of what Prof. Credner, in a letter to me lately, has characterised as fanciful ideas ('phantastische Ideen') on 'general and regional metamorphism.'

NOTE Q. (cf. p. 80.)

The formation of minerals of the Spinell family in metamorphosed limestones at a dry heat has received illustration from the observations of Stelzner and Schulze at Freiberg (Credner, *Elem. der Geol.* 6th ed. p. 313). The walls of the clay muffels used in the reduction of ZnO were found converted into a mass of *zinc-spinell* and tridymite, although these walls had remained solid (but porous) at a temperature of 1300°C for a week together. The zinc would appear to have penetrated the walls in the form of vapour, along with (probably) dry steam. This seems to point to a process of the nature of 'fritting' in the synthesis of the spinell, the Al_2O_3 being taken up at high

temperature by the oxides of zinc, iron, (and magnesium.) We can understand the formation of Zn O by the reducing action of the hot zinc-vapour both upon $Fe_2 O_3$ (of the clay) and $H_2 O$, thus

$$Zn + Fe_2 O_3 = Zn O + 2 Fe O;$$
$$Zn + H_2 O = Zn O + H_2.$$

With the latter we are familiar.

Then, by a fritting process, the $Al_2 O_3$ would be taken up by the ZnO, FeO, (and Mg O, if present) from the silicates of alumina contained in the clay, the $Si O_2$ being set free, and crystallizing as tridymite. In such a case $H_2 O$ could clearly be present only as a dry gas, and laboratory-experience leads us to suspect that the spinells were all formed in the dry way at high temperatures, since the soluble aluminates are very unstable in the presence of carbonated water. (Roscoe and Schorlemmer, *op. cit.*, vol. ii, part i, p. 443). I have observed that such an aluminate as that of potassium (which is formed when hydrogen is made by the action of aluminium upon potassium hydrate) undergoes decomposition in a day or two, an amorphous earthy powder of alumina being precipitated. The 'earthy dust' of authors may result in some cases in the archæan rocks from the action of water upon aluminates of the alkalies (and perhaps also of the alkaline earths) which were first formed along with the insoluble aluminates of less strongly-positive bases (the spinells). It is conceivable that this may have been a pretty fruitful source of free alumina (first produced by direct oxidation of aluminium), in the form of amorphous oxide or hydroxide, or as subsequently taken up by $Si O_2$ to form silicate of alumina under suitable conditions. The facts seem to lend themselves to the idea that a considerable proportion of the alumina of the crust of the earth may have been in the first instance appropriated in the dry way by the bases of the alkalies, as aluminates, and that the nascent Al_2O_3 furnished by the decomposition of these aluminates was thus in a condition specially favourable for combination with $Si O_2$.

It is an interesting point to note here (cf. p. 12 of this work) that on melting

$$\left.\begin{array}{l} Al_2O_3 + 3Na_2O = Na_6Al_2O_6 \\ \text{but } Al_2O_3 + K_2CO_3 = K_2Al_2O_4 + CO_2 \end{array}\right\} \text{Wislicenus (}\textit{Anorg. Chem.}\text{, § 719);}$$

from which it appears that at a glowing heat Al_2O_3, like SiO_2, can expel one molecule of CO_2 from the alkaline carbonates, uniting with the base to form an aluminate, as SiO_2 does to form a silicate.

NOTE R. (cf. p. 96).

Fahlbänder.

The possible origin of these suggested in this work removes the necessity for regarding the formation of the great beds of iron-ore found in them as due to the intervention of the action of organic matter, and their presence therefore as affording evidence of the existence of vegetation in archæan time (see Sterry Hunt, *Chem. and Geol. Essays*, pp. 13, 301, quoted by Etheridge in his Presidential Address to the Geological Society in 1881). The fallacy is repeated by Geikie ('Text-book,' p. 639, ed. 1882). The direct product of combustion of iron-vapour is $Fe_2 O_3$. This is a matter of direct observation when a steel watch-spring is burned in a jar of oxygen. There are two oxides formed, $Fe_3 O_4$ which falls down in molten masses, and $Fe_2 O_3$ which is given off by the flame (*i.e.* the burning vapour, the iron being partly volatilized by the intense heat of combustion, as the wax is in an ordinary candle-flame), the latter condensing as a brown sublimate on the walls of the jar. Three of the most noteworthy facts in connexion with the Fahlbänder are (1) the quantity of free $Si O_2$ often present as quartz, (2) the abundance of magnetite, (3) their frequent inter-bedded relation with hornblendic schists. (1) and (3) seem to suggest the deficiency of good alkaline fluxing materials to promote combination of the oxides of iron and the silica by fusion, while the magnetitic form of the iron-oxide suggests the action of iron-vapour at a

high temperature upon steam, as in a well-known laboratory-process. On the other hand it is possible that some of the phenomena presented by the Fahlbänder might be explained by the hypothesis of local rapid cooling by the down-rush of great masses of cooler air and vapours somewhat as appears to happen in connexion with 'sun-spots,' and compared with which the greatest atmospheric disturbances within the range of human experience are probably mere trifles.

Note S. (cf. p. 92.)

Relation of Organic Developement to Physical Environment.

A comparison of Mr. Etheridge's analytical tables of palæozoic organic remains in his Address to the Geological Society in 1881, leads to one or two conclusions of great theoretical interest. We note the absence throughout the earlier palæozoic formations of (1) plant-remains (with a few doubtful exceptions,) (2) air-breathing animals, (3) fishes. May it not be that the atmospheric conditions were as yet unsuitable for the existence of such forms owing to the presence of a large excess of CO_2 in the atmosphere (and therefore in solution in the waters of the ocean) and a corresponding deficiency of oxygen? The appearance of fishes in the Ludlow period, and the rapid increase of that class in the Devonian seems to suggest that at about the close of the Silurian period those conditions were being gradually reversed, until in the Carboniferous period the quantitative composition of the atmosphere (and consequently the aëration of oceanic waters) permitted the first appearance of Amphibia along with 290 species of fishes and 339 species of plants (mostly vascular cryptogams). The plentiful supply of the food-stuff of plants (CO_2) as compared with the proportion of that gas present in the atmosphere of later periods (though no longer in such excess as to interfere with their vitality) may well explain the enormously rapid assimilation of carbon (as has been suggested by Prestwich) which resulted in the formation of the Coal;* and obviously such an enormous developement of plant-life, as the great agency for the storage of carbon in the earth's crust, would tend to a rapid purification of the atmosphere, so as to render it fit for the support of air-breathing forms, which appear to have increased rapidly in mesozoic time. The absence of land-plants in the earlier palæozoic periods might be perhaps accounted for by the absence of dry land; but we cannot account for the absence of fishes in the same way. Dr. Selwyn (whom I had the pleasure to meet at Birmingham when the British Association met there in 1886) stated to me in conversation that he thought it likely that there was no very general elevation of dry land before the Devonian period, though I do not recollect what the data were on which he based that view. But it seems to be supported by the facts just referred to, so far as negative evidence can go to prove anything. If however we recollect what has been urged of late as to the great magnitude of the earlier oceanic tides, we see that in earlier palæozoic time any elevated region would have to be many times higher above mean sea-level than was required in later and recent times in order to form dry land. It may be therefore that though such great archæan land-surfaces as that of North America have never been permanently submerged as a whole since their first elevation in later archæan time, yet that elevation (being regional)† was probably very gradual and permitted the rolling of great tides over them for ages, with the consequent proportionally greater scour and mechanical disintegration of the rocks. In this way some light seems thrown upon the history of the Cambrian conglomerates.

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* See also note by the author on 'The Relation of the Percentage of CO_2 in the atmosphere to the Life and Growth of Plants.' (Brit. Assoc. Report, 1888, p. 661).

† See Credner, *Elem. der Geol.* pp. 189, 90; also Suess, *Entstehung der Alpen* last chapter '*Antlitz* (features) *der Erde*,' and Heim, *Mech. der Gebirgsbildung.*

NOTE T.

The Airolo Series.

It may be well to consider here a thorough-going investigation of a complexus of shales, quartzites, and impure limestones, which Dr. Ulrich Grubenmann of Frauenfeld has recently made.* The series has been (apparently) enfolded in a sharp syncline between the two enormous gneissic massifs of the St. Gotthard on the north, and the Tessina group on the south.

We know—on pretty sure evidence—that the series of movements from which the present Alpine system has resulted, began (at the latest) before the end of palæozoic period. Such movements could hardly take place without a certain degree of flexure in the archæan gneisses and schists. Some of these synclinal flexures would be for a long time below sea level, as the Airolo-syncline must have been, if these later sediments (as the Swiss geologists believe) are of marine origin and of the ages of the Trias and the Jura. These sediments have undoubtedly undergone in the later mountain-building stages, an enormous squeezing and squashing, as they have been folded back upon themselves, so as to compress them (according to Grubenmann) into less than $\frac{1}{4}$th of their original dimensions, the synclinal flexure having been accentuated into a sharp synclinal fold ('Mulde'). Here we find such distinctly individualized minerals as *garnet*, of which however no analysis is given†; *disthene* (Al_2SiO_5) with its variety *kyanite*; two micas, *margarite* (a lime-mica approximating in composition to the formula $Al_4CaH_2Si_3O_{12}$), and *meroxene* (a magnesia-mica, a variety of biotite, of a much more complicated structure than the former); *quartz* in grains and layers; *tourmaline* in small needles; *rutile*; *zoisite* in small prisms; *zirkon* (occasionally met with) in rounded elongated grains; *magnetite; calcite*, often so intimately associated with quartz as to point to the reaction of CO_2 upon $CaSiO_3$ as its probable origin; all these occurring in a series of schists intercalated with quartzites, where the field evidence shows that the rocks have been subjected to a maximum of pressure.

Are we bound to follow Grubenmann and others in attributing all these results to 'dynamo-metamorphism'? Hardly; and that for the following reasons:

(1). As to the *age* of the sediments. They are assigned to the age of the Trias and the Jura because the lower strata contain much gypsum and dolomite, and in the garnet-bearing schists, belemnites are said (on the authority of Studer) to occur at Fontana, and on the Nufenen Pass between the Val Bedretta and the Upper Rhone Valley, while in rocks in Val Piora, which are of similar nature to the 'Kalkglimmerschiefer' (which constitutes by far the largest mass of rocks entrapped in this synclinal fold), it is said that Escher V. d. Linth found 'undeterminable but undoubted organic remains' (unbestimmbare aber unzweifelhafte organische Reste). The first of these facts, the occurrence of gypsum and dolomite, can hardly count for much in determining the age of the rocks; if the belemnites are not better preserved than those said to occur in similar schists in specimens exhibited by Prof. Heim at the International Congress, they can afford at best but dubious evidence; and, lastly, the collapse of the theories which have been built upon the supposed organic origin of the structure known as Eozöon Canadense

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* 'Ueber die Gesteine der sedimentären Mulde von Airolo'; *Mitth. der Thurg. Naturf. Gesellschaft*, Heft viii, (Frauenfeld; Hubers, 1888.)

† This is much to be regretted, since a comparison of their composition with those of neighbouring archæan crystallines would have been of great value in the investigation.

ought to be a warning against attaching any serious importance to supposed 'undeterminable organic remains.' But apart from the question as to the age of the sediments, which is but a matter of secondary importance, we must consider

(2). The far more important question as to whether the garnets, disthenes and micas are *authigenous* or *allothigenous*. Grubenmann seems to assume the former to be case. Yet there are facts stated in his paper which should make us hesitate to adopt this view. For the garnets are in many cases flattened by pressure, and the appearance of the garnetiferous schists of a similar history which Dr. Heim exhibited in London in 1888, seemed to me hardly reconcilable with an authigenous origin for the garnets at least; so much so that I made a suggestion to him to that effect. Dr. Grubenmann, moreover, tells us that as the succession of the inverted schists is followed up the mountain-side in the Airolo district, the highest zone (with two micas) "passes over into the amphibole- and garnet-bearing schists of the southern schist- and gneiss-zone of the Gotthard-massif;* a fact which points strongly to the *derivation of the materials of these altered shales of the anti-clinal fold directly from the archæan schists and gneiss*, the two series having become welded together by the pressure which has induced the 'schistosity' observable in the sedimentary series.† There is another strong fact against the authigenous origin of the contained minerals; that is to say, *most of the minerals contained in the 'Schiefer' occur also enclosed in the gypsum*, which is found near the base of the series. Quartz, pyrite, mica, talk, tourmaline, disthene, and zirkon are mentioned as occurring embedded in the gypsum (anhydrite), out of which they could scarcely be fabricated. Disthene we know is a very stable mineral and endures mechanical transport by water to almost any distance without being decomposed, since it is found in "clear rolled crystals in the gold-washings on the south side of the Urals" (Naumann); and Mr. Dick has recently found kyanite along with zirkons, rutiles, and tourmalines, in the Bagshot Sands of Hampstead. (*Nature*, vol. xxxvi, p. 91). Teall has also recently described rutile as occurring in some clays. Quartz, rutile, tourmaline, iron oxide, pyrites and calcite are constituents of Thonschiefer.‡ In the case before us the orientation of the micas, the disthene, and the rutile points to their pre-existence in shales, whose lamination planes have initiated the 'schistosity' subsequently induced by pressure. *(Supra*, pp. 60, 61.*)*

(3). If we admitted that some of these minerals are authigenous, and that pressure had given them in some cases their *crystalline form* (an essential factor certainly to their individuality *qud* minerals), it by no means follows that their present *chemical composition* is the result of molecular changes, caused by the intense pressure which has induced a schistose structure upon these sediments. The gypsum was pretty certainly precipitated as the result of chemical reactions in the waters of this confined and narrow area, as was also probably the dolomite (see pp. 9-10 of this work); a large proportion of the carbonate of lime might have been also precipitated, resulting from such reactions as are instanced on p. 6 of this work, which is the more probable from the fact which we may fairly assume that most of the water drained into the quondam area of sedimentation was received from regions consisting of rocks more or less rich in felspar; and the occurrence of $CaCO_3$ in some of these rocks intimately associated with SiO_2 seems to point to the

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* It may be well to note here that Grubenmann seems to have no difficulty in distinguishing these two series of 'Schiefer' from one another.

† The neighbourhood of the great intrusion of granite of Pitz Rotondo and Pitz Pisciora ought not to be overlooked.

‡ Kalkowsky *(op. cit.*, pp. 257-8*)*. The determination of the authigenous or allothigenous origin (in rocks) of tourmaline is (he remarks) well-nigh impossible, since these tourmalines occur with the same properties in the phyllites and mica-schists as well as in sandstones, conglomerates, &c.

probable reaction of free CO_2 upon silicates containing CaO. As for the lime- and magnesia-micas, it would be difficult to deny that they may have been formed from muscovite, derived mechanically from the neighbouring archæan rocks, by such reactions upon it of the salts contained in the sea-water, as are suggested on p. 29 of this work, the richness of these waters in salts of CaO and MgO being evidenced by the gypsum and the dolomitic rocks. Again, in the decomposition of the felspar, which must have formed a portion of the detritus of which these sediments are composed in order to furnish their argillaceous materials ('Thon'), it is at least rational to suppose that some of the silicate of alumina may have acquired a more basic character, such as is indicated by the formula Al_2SiO_5, which, as the analyses show, represents the composition of the disthene here. On the other hand, the direct derivation of disthene from the rather unstable mineral zoisite involves a comparatively simple reaction in the wet way:

$$\underbrace{Ca_4Al_6H_2Si_6O_{26}}_{\text{Zoisite.}} + 4CO_2 = 4CaCO_3 + H_2O + 3SiO_2 + \underbrace{3Al_2SiO_5}_{\text{Disthene.}}.*$$

That this is no vagary of the chemist is proved by Dr. Grubenmann's own observation *(op. cit.* p. 15*)* of the passage (in the weathered rock) of zoisite into $CaCO_3$, SiO_2, and (in some cases) disthene in the cracks and fissures of the zoisite itself.

Substitute in the above reaction K_2CO_3 for free CO_2, and we should get the reaction for the conversion of zoisite into white potash-mica, which is a matter of observation (Roth, *op. cit.*, p. 352). If such changes, of a purely chemical nature, took place in the fine silt derived from the adjacent siliceous rocks *antecedently to the subjection of the sedimentary mass to the pressure* which has given to these rocks their present schistosity, it is clear that we should have no right to attribute them to the operation of that pressure; even though we admitted that this might operate upon materials of the right chemical composition (previously formed by ordinary chemical reactions) so as to impart to them a crystalline structure and form. (cf. pp. 54-55 of this work).

"It is easy [as Dr. Grubenmann sagaciously remarks] to be tempted to allow too bold a flight to hypothesis in the region of rock-genesis"; and our only safe rule is to give hypothesis no place until we have exhausted all the resources which the ever-active known principles of hydro-chemical action can furnish towards the explanation of the facts observed. In the present instance of extreme mechanical action within a very limited and confined space, we have probably the ultimate results of the action of pressure upon materials previously prepared for it; the whole observed phenomena being not the result of pressure merely, but rather the resultant of the combined factors of time, hydrochemical change, physiographic conditions,† and pressure, with its concomitant metataxis, and perhaps in some cases metatropy.

(4). On general grounds it may safely be asserted that conditions of high temperature are those favourable to the building-up of the more complex silicates; while at lower temperatures and in the presence of water the tendency is towards the resolution of these into simpler or proximate constituents. The one is *synthetic*, the other is *analytic*. The vast difference in the degrees of probability of these two processes is at once appreciated by the chemist; and the experience of the chemical laboratory is on this point in

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* See Naumann-Zirkel's 'Mineralogie.' Graphically we might represent the composition of disthene thus:

$$\begin{matrix} Al{=}{=}{=}O \\ \quad\rangle(SiO_3) \\ Al{=}{=}{=}O \end{matrix}$$

the radicle (SiO_3) being the exact analogue of the (CO_3) of the carbonates.

† Anyone who has taken careful note of the nature of the detritus of modern valleys in the gneiss and schist regions of the Alps will appreciate the importance of this consideration.

perfect accord with observations in the field and under the microscope, as the study of secondary paramorphism soon reveals to us. The tendency of zoisite to undergo decomposition (which is pointed out by Dr. Grubenmann), the much greater abundance of disthene than of zoisite in these altered shales, the decomposed state of the garnets* and of much of the zoisite which remains, the way these two minerals (as also some of the others) are fractured and water-worn, the frequent accompaniment of these garnets by such well-known decomposition-products of garnet, as iron oxides, and quartz, (which commonly separate out as the lime of a garnet is removed in solution and the iron gets oxidised from the protoxide into the peroxide), and a comparison of these facts with numerous instances which Justus Roth† has collected of the formation of decomposition-products from garnets, the general occurrence of clay and carbonaceous matter in the most highly-altered of these shales, the fractured and worn condition of many of the quartz-grains, seem to furnish strong cumulative evidence of a very considerable period of exposure of the most characteristic minerals of these thin shales to conditions inducive of chemical change, before the enormous pressure, which has given their present structural character to these altered shales, quartzites, and limestones, was brought to bear upon them. The absence of any record of felspar in these sediments is significant, as pointing again to a vast period of hydro-chemical action upon the original detrital minerals; and the probability of this is heightened by the single reflection that—if the Swiss geologists are right in assigning these altered shales to the age of the Trais—we have in this Airolo district a very insignificant series of shales and quartzites, not approaching—it appears—anything like 50 metres in thickness, to be regarded as the *equivalents in time* of the Triassic Dolomitic Series, which in the Eastern Alps is measured by thousands of metres.

(5). The succession of the strata under consideration is by no means certain, as Dr. Grubenmann candidly admits; the field-evidence, owing partly to alluvial deposits, and partly to the appropriation by a dense vegetation of the rich soil furnished by the weathering of the rocks themselves, being altogether insufficient for determining this with certainty. But this uncertainty leaves the broad distinction between these localized rock-phenomena and the great archæan series of gneisses and schists as clear as ever, except perhaps to those who only know Alpine geology through the medium of books.

A glance at Studer and Escher's Geol. Map of Switzerland, is almost enough to show one that the principal major longitudinal line of flexure in the Central and Western Alps is that which commences with the valley of Chamounix, (containing the Vernayaz series referred to above‡), is continued along the valley of the Upper Rhone (all that part above Martigny), the Urseren Thal, and the valley of the V. Rhein down to Chur. On general grounds, and taking into account the distribution of the 'Verrucano,' and 'T. Anthracit' along this zone of the Alps, there would seem to be little room for doubt that the initiation of this line of flexure dates from Palæozoic time; nor am I aware of any facts in the geognostic structure of the Alps which militate seriously against such a supposition. The protrusion of the great granite-gneiss-schist massif of Pitz Rotondo and St. Gotthard, has caused a bifurcation of this great line of flexure as we trace it eastwards, and has given us, as a consequence, the line of flexure of the Val Bedretto and the Val Leventina, which is deflected still more to the south by the great crystalline *massif* of the mountains which furnish the head-waters of the H. Rhein §

* So much so that the garnet could not be separated from the accompanying minerals in sufficient purity for an analysis to be made.

† *Allgem. und Chem. Geol.*, pp. 352-363. See in particular Lemberg's experiments on Grossular (p. 363). ‡ See page 90.

§ I am of course aware that Dr. Heim considers the architectonic structure of the Alps to have played only a very insignificant rôle in determining lines of erosion and denudation, but in this I am unable to follow even so great a master.

As narrow zones of sedimentation they must date from a very early stage in the building of the Alpine system, and the nature of the sediments formed here would differ (as pointed out above) presumably from those formed in more extended basins, so as to afford special facilities for pressure to impart to them such a 'schistosity' as may well simulate true foliation. There is no question here—it appears—of the grey schists (schistes gris = Bündner Schiefer) which form a narrow zone all along the south flank of the Val Bedretto, but only of a very limited series, their exposure in the section worked out by Dr. Grubenmann, not amounting even when reduplicated by overfolding (in part) to more than a very small fraction of the whole section, in which impure limestones and quartzites predominate.

NOTE U.

On the Developement of the Archæan Crystallines. *

"The stratified sedimentary formations (palæozoic and neozoic) form a great series, whose origin and present developement we can for the most part explain with apparent certainty. But besides these there exist at the base of our formations vast series of strata of gneiss, mica-schists, phyllites, hornblende-schists, chlorite-schists, talc-schists, and granulites, interstratified with which occur granite-gneiss, marble, graphite, and iron-ores, as to the mode of developement and building-up of which the views of geologists are still not merely confused and indefinite, but to some extent widely diverse from one another.† The originally *sedimentary* mode of developement of all this complexus of archaic gneisses and schists cannot be doubted: in their stratiform differentiation, in their massive structure, and in their parallelism, which dominates the whole series, we are presented with a stratification (Schichtung) as real as in the fossiliferous series of clay-slates, limestones, shales and sandstones: concordantly with their distinctiveness as stratified masses frequent interstratifications (often on a diminutive scale) of the most diverse kinds of rock repeat themselves; between gneisses and mica-schists regular layers of granular quartzite and of conglomerates are met with; with the greatest regularity gneisses and granulites of the most manifold varieties are interstratified with chlorite-, talc-, mica-, quartzite-, and hornblende-schists; between these may occur layers of granite-gneiss, beds of crystalline limestone, magnetic iron-ore, and graphite, as well as complexes of graphite-schists; finally the gneisses pass into mica-schists, these into phyllites, and these again, so far as the stratification-order indicates, into the lower palæozoic fossiliferous formations: the phenomena as a whole indicating some *sedimentary* mode of developement of the gneisses and crystalline schists. Their present rock-character is, however, according to the view of many geologists, not original, but rather a change, a metamorphism has taken place in the course of time in the originally *clastic* material, out of which have arisen the crystalline structure and the petrographical habit which the gneisses and the crystalline schists at present exhibit. This process of alteration has been termed 'general metamorphism' or 'regional metamorphism.' As to the nature and origin of it the views however differ widely. By some it is regarded as the result of the active agency of the high temperature of the glowing internal mass of the earth; by others, as the result of intense pressure; by others again, as the result of hydrochemical processes, that is to say, of chemical processes carried on through the agency of fresh water penetrating to depths. In other words it is regarded, on the one side as a reaction of vulcanicity, on the other as atmospheric in origin.

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* Translation of § 13 of the 'Petrogenetische Geologie' of Credner's 'Elemente,' 6th edition, (Leipzig, 1887).

† J. Roth, 'Ueber die Lehre von Metamorphismus aud die Entstehung der krystallinischen Schiefer,' Berlin, 1874. G. W. Gümbel, 'Ostbayer. Grenzgeb.' Gotha, 1868.

Plutonic Regional-Metamorphism.

"According to the views of the Huttonian school, and especially more recently of Lyell and von Cotta, the metamorphism of the originally sedimentary material is the result of a long-continued heating through the internal heat of the Earth, whereby, under the influence of the simultaneously existing pressure of the overlying formations, a melting of the lowermost rock-masses concerned and an intimate recrystallization with transfer of material and reconstruction have come about. In this process an important part is assigned on the one hand to the waters which originally filled the interstices of the sedimentary rocks, owing to its increased efficiency as a vehicle of heat by convection and as an active agent in the solution and decomposition of mineral-matter in the super-heated state; on the other hand, gases and vapours developed in the glowing-liquid kernel of the Earth and driven under pressure through the rocks are considered important factors in metamorphism. In plutonic regional metamorphism the processes would thus be similar to those of contact-metamorphism in the neighbourhood of eruptive rocks, except that they would arise from an universally-active source of heat, the glowing interior of the Earth. Since now the metamorphic influence of the latter diminishes in intensity from the interior towards the periphery, the most deeply seated are by it affected and transformed in the highest degree. On account of this the gneiss occupies the lowest position, and above this follow in succession the mica-schists, the chlorite-, talc-, and hornblende-schists, and lastly the phyllites so closely resembling the shales (Schieferthon). There would follow however instead of a mere partial softening or recrystallization a complete fusion of the originally sedimentary rock-material, so that this would, like our volcanic lavas, be forced in the plastic condition into clefts and fissures to solidify as granite and syenite. According to this view the eruptive rocks had their origin in the upper zones of the earth's crust which once formed the bed of the ocean.

"This theory presumes that in a higher zone, that is to say, in that which included the sedimentary deposits of the primœval ocean, considerably high temperatures were produced from within the sphere, and explains this as the result of elevation of the surface of the Earth through the deposition of vast stratified systems, so that the isothermal planes of temperature were transferred upwards. In this way, by the filling-up of an ocean of over 10,000ft. in depth, through the deposition of a mighty complex of stratified rocks, the temperature of the originally superficial formations was raised to about 100°C.

Mechanical (tectonic) Regional-Metamorphism.

"In some places, always however in regions very limited in space, it has been established as a fact, that the clastic normally-constructed strata, where they have undergone specially intensified pressure, in the process of mountain-building, and thus exhibit complicated disturbances of their stratification, have taken on at the same time a crystalline habit. Observations of that sort have afforded the tempting opportunity to refer the crystalline schists generally and collectively to practically similar processes, and the developement of their crystalline character to the energetic thrust, folding, and pressure, which originally clastic strata have suffered, together with the heat generated by the accompanying friction and pressure. Indeed, so far has this idea gone, that the archaic gneisses and granulites extending through whole regions have been accounted for as eruptive rocks metamorphosed by pressure into schists along with the rocks which are subordinately interstratified with them. All these hypotheses as to such a widespread mechanical metamorphism prevailing similarly through vast areas and resulting from the pressure concerned in mountain-building, dispense however with convincing evidence for their data, and stand in part in direct contradiction to known facts. They are disproved by the simple fact, that many highly crystalline archaic regions exhibit in the highest degree a simple little disturbed architectonic structure, while the

neighbouring Silurian and Devonian areas, in spite of powerful folding, crumpling, overthrusting, crushing, and transversal cleavage have retained their original structure unchanged, as slates, (Thonschiefer), grauwacke, sandstone, and ordinary limestone.

Hydrochemical Regional Metamorphism.

"In contradistinction to these views as to the origin and process of metamorphism by plutonic action of heat or by pressure of whole systems of stratified rocks, the hydrochemical theory of general metamorphism, as it was especially taught by Bischof, recognizes in the saturation of the rocks with water through a vast period of time the cause of the metamorphism of rocks on a large scale, and ascribes to this a substantial alteration and recrystallization of metamorphosed strata. According to it the process consists in the transference to depths in the Earth of the materials taken up in the superficial strata of the Earth's crust, by the chemical activity of water in decomposing and dissolving mineral substances. This water, holding carbonic acid and oxygen, penetrates, after its precipitation from the atmosphere, through the rocks near the surface, where the oxygen it holds is taken up in processes of oxidation and the carbonic acid is removed from it by the decomposition of certain silicates, until both gases, after the water has penetrated to greater depths, are used up entirely and so these reactions must cease. When these waters laden with the soluble mineral-substances arrive at the more deeply seated strata, they serve as agents in the alteration of rocks. The alkali and lime-silicates transported below in this fashion combine with the alumina- and magnesia-silicates already present there to form compound silicates *(e.g.* felspar, mica,) which, since this process goes on exceedingly slowly, separate out in the crystalline form. Besides single silicates which combine with one another, silica is present in larger quantities than is required for the resultant double silicates, and so in this process there is a separation-out of quartz. Thus the hydro-chemical metamorphism of rocks consists in the introduction of mineral-solutions from the more superficial zones into the deeper-seated; further in effecting *pari passu* combinations and decompositions between those solutions and the rock-materials which are saturated with them; and lastly, in the resultant formation of new minerals which, on account of the slowness of the process, crystallize. The result is the complete transformation of the chemical composition, of the petrographical constitution, and of the structural habit of the original rock. This process, even when aided by the pressure of the overlying strata and by the increase of temperature at depths, requires enormous time. The direct consequence of this theory is that *(e.g.)* from one and the same limestone, according to the nature of the circulating mineral-solutions and the chemical processes induced thereby, in some places a pyroxene- or amphibole-rock, in others a garnet- or epidote-bearing rock, in others again, a quartz- or felspar-rock may be developed.

"A capital objection to this theory of hydro-chemical metamorphism consists in the fact that it requires an enormous period of time, longer even than that which has elapsed from the beginning of the palæozoic ('Silur') to the present, for the complete morphological transformation of the rocks through saturation with water; since all the formations from the Silurian down to the most recent ore found, where their *normal* construction is displayed, not yet in the condition of metamorphosed rocks.* On the contrary, all the palæozoic formations, and especially so the Cambrian and Silurian stratified systems, which follow immediately upon the crystalline schists, contain rolled fragments both of gneisses and of crystalline schists, which possess exactly the same structural character as their parent-rock (Muttergestein). The hypothetical long-continued process of hydrochemical metamorphism of the pre-Cambrian formations was therefore already completed at the entry of the Earth into the palæozoic stage of its developement (in die silurische Periode), and could not

* Compare § ii. pp. 16-17 of this work.

have taken the long time demanded for it. Were this really the case all our lower palæozoic formations must long ago have been transformed into crystalline schists and gneisses.

*Originally crystalline developement and Diagenesis.**

"Besides the grounds of objection alleged above a few geologists have perceived a number of other forcible objections, which compel them to hold that upon the whole such general metamorphism as the earliest sediments have undergone was of a special character; nay, more, that their rock-character which we see in them to day may be primitive and original;—original in the sense in which this word is used of the shales, conglomerates, sandstones, marls and limestones. Of these objections, in addition to those alleged above, the following may be urged:

"(1). Everywhere, where the group of the archaic formations is known, whether it be India or Scandinavia, in Canada or in Bavaria,† it is made up of the same members, possesses the same petrographical structure, producing in the individual parts the same accessory constituents, presenting in its petrological variations the same different kinds of rock. Such a complete resemblance in the petrographical character of the 30,000 metres of strata comprehended in it, cannot possibly be the product of an alteration and of the accidents of a process of crushing or saturation with water.

"(2). The variations in the nature of the members of the gneiss- and schist-series both in the thin layers and in complex stratified masses, are in complete accord with the differentiation of the strata; the two things have a mutual dependence on one another. In this respect the successive changes in the materials correspond with alterations of conditions which have determined their stratification. On the other hand, had their structure resulted from a mechanical or hydro-chemical metamorphism we must have had confused assemblages of rocks of different character, and not sharply-defined stratification planes, often only a few centimetres apart of rocks quite different in their habit.

"(3). The general parallelism of the arrangement of the mica-laminæ and the hornblende-prisms in the mica- and hornblende-schists, the adhesion of the mica-lamellæ to accessory minerals (*e.g.* garnet) can be very well explained by an original developement of this mineral [in the genesis of the rock], not by a later developement out of solid rock-material.

"(4). If on correct grounds it is accepted as an established fact that the individualization of the felspar-crystals of the porphyries and the trachytes, and the fracture and separation of the fragments of the orthoclase and sanidin-crystals had already taken place before the solidification of the intervening ground-mass, the same inference may be fairly drawn with reference to the accessory crystalline individuals of hornblende, tourmaline, garnet, spinell, staurolite, apatite, etc., as they occur in the gneiss- and schist-formations; since here also fractured portions of tourmaline-, zirkon-, and garnet-crystals occur pushed aside from one another and enclosed in the rock-mass.

"There is moreover no single observed fact in the largest region of the pre-Silurian (pre-Cambrian) crystalline-schist series in the whole of Germany (that is to say, in the Ezzgebirge and in the mountains of the East-Bavarian frontier), which answers to the supposed developement of the phyllites (Urthonschiefer), the mica-schists, and the gneisses by any later metamorphic

* G. W. Gümbel: *Ostbayer. Grenzgebirge*, 1868, p. 833. See also p. 166.
H. Credner: *Gliederung der vorsilur. Format. Nordamerikas* (Zeitsch. f. d. Ges. Nat., 1868. Nos. 11, 12, p. 353; *Neues Jahrb. f. Min.*, 1870, p. 981.
Pfaff: *Allg. Geol. als exacte Wiss.*, 1873, p. 145.

† According to Mr. Macpherson, the same regularity of succession is seen in the crystalline series of the Spanish Peninsula. (See *Prôces-Verbal* pour le 19 Septembre, of the Int. Geol. Congress of 1888).

process, in either the plutonic, mechanical, or hydro-chemical sense of the word. Much more do the constant transitions of the different rock-groups of these primary mountain-masses along their whole parallel bounding surfaces, with the identity in kind or close resemblance of their accessory constituents, of their variations in texture and in the intermingling of their essential constituents, as they recur similarly in the corresponding strata, plainly show, that the members of the archaic group of formations are the product of an universally direct developement (das Produkt allmählicher direkter Ausscheidung.) * * * * * * * * * * * *

"In the regular succession of the gneisses by the mica-schists, of these by the phyllites, and finally of the last-named by the fossiliferous palæozoic slates (Schiefer), we should have, according to the theory which regards the morphological character of the archaic series of strata as primordial or original, only to take into account a process of differentiation changing materially with time, and a difference of external conditions determining the developement of the separate masses, a gradual diminution of conditions favourable to simple chemical change,* and an increased action of mechanical and organic agencies in the formation of rock-material, along with which *(pari passu)* a gradual falling-off of the tendency to the separation-out of crystalline rock-constituents has come about."

[This general conclusion of Dr. Credner's agrees, it will be seen, substantially with that at which I had previously arrived from my own lines of investigation, and the agreement is very gratifying to me.]

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* That is to say, as the result of 'dissipation of energy.'

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ERRATA.

p. 16 (foot note*)
for '2 $NaSiO_3$,' *read* 2 Na_2SiO_3.

p. 32, line 20—
for 'App. ii,' *read* App. ii, Note G. [The curious mineral referred to requires further investigation. It contains Al, Ca, and Ba, and a trace of Fe (as bases), while, along with SiO_2, some sulphuric and phosphoric acids are also present. All these exist in the unfused clastic matrix of the rock.]

p. 37, line 17—
for 'in' *read* into.

p. 54—
The asterisk has dropped out of the foot-note.

p. 80—
(foot-note*) for 'taken of' *read* taken off.

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